THURSDAY, DECEMBER 5, 1878

BOTANICAL CHEMISTRY

The Organic Constituents of Plants and Vegetable Substances and their Chemical Analysis. By Dr. G. C. Wittstein. Authorised Translation from the German Original, Enlarged with numerous Additions, by Baron Ferd. von Mueller, F.R.S. (Melbourne: McCarron, Bird and Co., 1878.)

MANY who have been interested in botanical studies may have been struck with the varied nature and great number of chemical substances which are extracted from plants and which in many cases impart to the flowers their special and characteristic brilliant colours. Up to the present time we have had in English no work which has devoted itself specially to the systematic description of the nature and preparation of such substances; and the literature in connection with the subject, which is by no means meagre, with accounts of investigations in chemico-botanical research must be sought for in journals generally devoted to purely chemical matter, and consequently less likely to attract the attention or be available for the use of the more general reader.

With regard to other countries, however, this has not been the case, as we find a considerable portion of the work of Berzelius devoted to a consideration of the bodies found in plants, and the "Anleitung zur Analyse von Pflanzen und Pflanzentheilen" of Rochleder has long been known both in Germany and elsewhere as affording a good collection of the results of investigations in vegetable chemistry up to the date of its publication about the year 1858.

During the last year, however, Baron Ferd. von Mueller has brought forward a translation of Dr. G. Wittstein's "Anleitung zur chemischen Analyse von Pflanzentheilen auf ihre organischen Bestandtheile," published in 1868, the value of which he has found in his own researches, and which he has for some time wished to render available for English readers.

The present edition consists of two parts, each divided into three divisions, which form as it were the chapters. In the first part the author has placed the consideration of the proximate constituents of plants and vegetable substances as far as hitherto known, together with their properties and mode of extraction. His first intention in the arrangement of this part, which is naturally the largest portion of the work, was to adopt as far as possible a systematic classification. From the imperfect state of knowledge, however, as to the exact constitution of the bodies, and from the fact also that some of the better known substances possess properties which might cause them to appear in several groups if a classification depending on natural properties was taken, he has finally adopted an alphabetical order and has thus formed a dictionary of so-called phytochemical substances.

In the consideration of the individual substances Baron von Mueller has evidently confined himself almost entirely to their preparation from natural sources, and we have therefore no description of the very interesting and remarkable synthetical methods which are now employed for the production of certain of these bodies. This may of course have been beyond the limit originally intended by the present editor, but we should hope in the event of future editions to have some mention made of the more important recent investigations in this direction. The two latter divisions of the first part are occupied with a synopsis of the plants which yield the bodies previously described and a list of the plants indicated, systematically arranged in their different natural orders. The first of these lists is remarkably good, as it gives not only the names of the plants and those of the substances which they yield, but also the various parts of the plant from which these latter may be extracted. The want of such a list has been felt, and this part of the work might have been extended to rather wider limits; in its present form, however, it will still prove of considerable use. In assigning chemical formulæ to the substances described in the first part of the book the editor has retained the older forms of notation, but has introduced immediately after the alphabetical list of bodies a table containing the molecular weights of the compounds described according to the modern views adopted by chemists; this necessity for two lists concerning the same thing introduces confusion in the mind of the reader, and it would be well therefore in a future edition to dispense altogether with the older forms of molecular weights as they are little used at the present day.

The second part of the work is devoted to the apparatus and reagents necessary for phyto-chemical analysis and to the description of a systematic course embracing the different methods of procedure in conducting such researches. In these analyses one of the most important points is the proper extraction of the various ingredients of the plant; for this purpose solvents such as ether, alcohol, and water are employed. As at first sight it might appear immaterial in what order these solvents are to be used the author points out the importance of employing them in the following order: first ether, then alcohol, and finally water, and by this means preventing such bodies as wax or fat which are completely and entirely dissolved by ether, from passing also into the alcoholic extract in which they are only partially soluble. This also would apply to the extraction of certain of the alkaloids, as in their case partial separation may be carried out in their extraction by the different solvents.

It is to be regretted that the more modern names and atomic weights are not employed in this portion of the work, and also in the description of the preparation of reagents; thus, we find the molecular weight of calcium carbonate given as 625, and that of calcium oxalate as 1025: at the present time the use of such numbers tends greatly to confuse the student.

At the end of the work Baron von Mueller has arranged some useful tables, comprehending the comparison of Centigrade and Fahrenheit thermometric scales, the specific gravity of alcohol of different percentages by weight and by volume, the relation between cubic centimetres and cubic inches, between litres and fluid ounces, and a table of the atomic and molecular weights of the principal elementary bodies.

There can be little doubt that this work supplies a great want in chemical and botanical literature, but there is still room both for the farther elaboration of the matter discussed, and, in certain cases, for some

improvement in the rendering into English of the matter already employed. In the direction of chemico-botanical research there is great room for investigation, and a text-book embracing the knowledge already acquired, and information on points in connection with the chemistry of vegetable physiology, would render such a work of interest not only to the scientific chemist or botanist, but also to the general reader. Baron von Mueller's translation forms an excellent nucleus for such a work, and should a future edition of the book be required, we should hope to find it enlarged in such directions.

J. M. T.

GEOGRAPHICAL ASTRONOMY

Abriss der praktischen Astronomie, vorzüglich in ihrer Anwendung auf geographische Ortsbestimmung. Von Dr. A. Sawitsch, nach der zweiten russischen Original-Ausgabe. Neu herausgegeben von Dr. C. F. W. Peters. (Leipzig, 1879.)

As may be inferred from the title of this work, the astronomical reader will not find it to be a general treatise on the practical branches of the science, but one confined to the theory and uses of instruments, and explanation of methods employed at the present day in the determinations of geographical positions. As such the name of its author, Dr. A. Sawitsch, the well-known Professor of Astronomy in the Imperial University of St. Petersburg, will give the work high recommendation in the estimation of the student. The two volumes of the original edition are now incorporated in one, and such modifications as have been rendered necessary by the introduction of new or improved forms of instruments, and refinements of observation and reduction have been introduced in a great measure by the author himself. In the opening chapter we have explanations of the various methods of reckoning time, and the transformation of one into another; the reduction of mean into apparent places, the calculation of refraction and parallax, and the influence of the earth's compression upon the geocentric co-ordinates of points upon the surface, with remarks upon angular measures in general, and upon the astronomical telescope and its adjustment, the microscopes, verniers, level, &c. In the first section, the author treats of the transit instrument, and enters into the various adjustments to which it is subjected, and also describes in some detail the universal instrument of Piston and Martins, and the errors of division to which instruments for angular measures may be liable. The second section is devoted to the determination of latitude and time by measure of zenith distance, of time from corresponding altitudes, &c. The third section enters more fully into the uses and theory of the transit instrument, and likewise describes Bessel's method for the determination of latitude thereby, supplying practical rules and an example. The next section treats of the determination of azimuth, and of the influence of diurnal aberration on the polar co-ordinates of a star. The fifth section contains a valuable outline of the various methods applicable to the determination of terrestrial longitude, including the telegraphic method, the transportation of chronometers, and longitude by observations of eclipses, especially those of the sun, and by lunar occultations.

The reference to the utility of eclipses for longitude-determination leads to an important chapter on Hansen's method for the calculation of the general circumstances of these phenomena upon the earth's surface, and the methods followed by Dr. Zech, in his researches on the historical eclipses; and, as a numerical example, the formulæ are applied to the computation of the circumstances of the total solar eclipse of August 18, 1887, to which frequent reference has been made in astronomical treatises. The data are founded upon the lunar tables of Hansen and the solar tables of Leverrier. Further, we have a discussion on moon-culminators in their application to longitudes, with notices on the methods of Nicolai and Struve, and a fully-worked-out example. The sixth section relates to the reduction of the longitude, latitude, and azimuth of a place to another, both accurately and approximately, and the determination of the distance of points on the terrestrial spheroid, of which the geographical positions are given. There are two supplementary chapters: the one bearing upon reflection-instruments, and of course entering at length into the use of the sextant; the other treating of interpolation, with special reference to the formulæ of Bessel and Hansen.

In the language in which this work originally appeared it would be almost a sealed book in Western Europe. The excellent translation into a language of which every scientific student should, in these days, possess a knowledge, now placed in our hands by Dr. Peters, will be, without doubt, a welcome addition to his means of instruction on an important branch of practical astronomy.

OUR BOOK SHELF

A Treatise on Dynamics of a Particle, with numerous Examples. By P. G. Tait and the late W. J. Steele. Fourth Edition. (London: Macmillan and Co., 1878.)

THE bibliography of this revised text-book is—a first edition in 1856, 304 pages; a second edition in 1865, 363 pages; a third edition in 1871, 428 pages; and the present edition of 407 pages. There are slight alterations in the disposition and amount of the matter in this edition, caps. x. and xi. of the third are put into cap. ix., caps. v. and vi. are contained in cap. v. of the fourth. The position of some of the exercises has been changed. The main features remain unaltered. The revision has had the advantage of Prof. Greenhill's supervision, who has verified (and corrected where necessary) the Examples and has freely introduced the use of Elliptic Functions. There is no need of any commendation for a text-book so well-known. We are, however, very much disposed to think that had Prof. Tait composed the work at a later date than he did, it would have differed somewhat from its present form and have approximated more closely to the Natural Philosophy brought out under the joint editorship of Sir William Thomson and himself. The author justly complains that "several sections in which some novelties appear have been translated almost *letter* for *letter* and transferred, without the slightest allusion to their source, to the pages of a German work. Several other books have obviously been similarly treated. It is well that this should be known, as the English authors might otherwise come to be supposed to have adopted these passages *simpliciter* from the German."

Familiar Wild Flowers. Figured and Described by F Edward Hulme, F.L.S., F.S.A. First Series. With Coloured Plates. (Cassell, Petter, and Galpin.)

SCIENTIFIC books are of three kinds: to inform the

scientific world of some fresh discovery or advance—works of research; to offer a digest, for the information of students, of results already attained—text-books; and to attract to the paths of science the outside public—popular works. The pretty and attractive book before us belongs to the last of these categories, and is, we think, well calculated to gain the end in view. It consists of chromo-lithographs of nearly fifty of our better-known native wild flowers, with two or three pages of gossipy talk about each. Of the letter-press not much more can be said than that it is fairly accurate from a botanical point of view, and pleasantly written. The illustrations strike us as unusually good of their kind. They have of course the inherent defects of this mode of illustration, in the absence of half-tones and delicate shades; but the general aspect of the plant is in nearly all cases well and faithfully given, and the drawing is good. The book is a very good one to put in the hands of a child to interest him or her in the wealth of wild flowers which is such a source of delight to all dwellers in the country who have eyes educated to see their beauty.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Telephone

I HAVE just read an article in NATURE, vol. xviii. p. 698, on the history of the speaking telephone, which contains an erroneous statement of facts which happen to be within my own knowledge; so that, in the interest of a truthful history of this discovery, it is incumbent upon me to make a brief statement in regard to it.

I had the honour to be one of the judges at the International Exhibition at Philadelphia, and of the group to whom was confided the examination of instruments of research and precision. In the performance of my official duty I took part in the experiments which first brought the speaking telephone to the notice of the scientific world. Prof. Bell and Mr. Elisha Gray were both exhibitors at that Exhibition. Mr. Gray's apparatus was conspicuously shown near one of the main aisles, with the exhibit of the Western Electric Company, while Prof. Bell's was in a side room in one of the galleries, as a part of the Massachusetts' educational exhibit. About the middle of June, 1876, Prof. Bell came to Philadelphia to give personal explanations in reference to his apparatus, and before any public exhibition was made he stated to me in detail the character of his inventions. He was working at two independent things, the one the multiple telegraph by means of transmitted musical notes, the other the transmission of articulate speech over long distances. I told him that I was present in May, 1874, at the rooms of Prof. Henry, in the Smithsonian Institution, when Mr. Elisha Gray exhibited to us an apparatus for the electric transmission of musical sounds, and I asked him whether his first invention was similar. He said there was some similarity, although each had worked independently, and that there was a dispute as to the priority of invention. While sanguine as to practical results from his multiple telegraph, his great invention was the speaking telephone, which he believed he had discovered, and in respect to which there was no rival claimant. He said the idea came to him from some of the suggestions in respect to sound vibrations made by Helmholtz, and that he had succeeded, after patient research, in constructing an instrument which would transmit articulate speech. To this invention he desired to direct the attention of the judges.

The experiments with the telephones had to be made when the Exhibition was closed to the public, and the first experiments were made by Sir William Thomson and others on Sunday, June 18 or 25—I do not now remember upon which of these two dates. Their Majesties the Emperor and Empress of Brazil were present at these experiments. Attention was first given to Mr. Gray, and he gave a lengthy account of his experiments, which had resulted in the perfected apparatus which he then exhibited. He gave an explanation of his various instruments in chronological order, and conducted some very entertaining experiments as he proceeded in his discourse. The object which he had in view was to send many messages simultaneously over the same wire by using sending and receiving instruments of different musical notes.

The greater part of the day was given to Mr. Gray, so that insufficient time remained for satisfactory trial of Prof. Bell's apparatus. The judges and the distinguished visitors present did, however, proceed to the Massachusetts gallery, and Prof. Bell explained briefly his two inventions, and some experiments were made with his speaking telephone, enough to excite the curiosity of those present in the highest degree. The results were so at variance with the views hitherto received that it was determined by my distinguished colleague, Sir William Thomson, to make other experiments, in which I took part. These experiments were made two or three days later, in the building known as the Judges' Pavilion, in the evening, after the visitors had left the grounds. Prof. Bell had returned to Boston, and was not present at this trial of his apparatus. It was brought over to the judges' pavilion, at my request, by Mr. Hubbard, one of the officers in charge of the Massachusetts exhibit, and the experiments were made by Sir William Thomson and myself. Every precaution was taken to make an impartial test. I was at the transmitting instrument which was placed out of doors at a distant part of the building, and Sir William Thomson was at the receiving instrument in a distant room in the building. After some experiments to find the pitch of voice which would suit the vibrating membrane then used, I received word by messenger from Sir William that he could then hear distinctly, and accordingly the pitch of voice then used was maintained in the subsequent trials. I held in my hand a copy of the *New York Daily Tribune*, and I began to read to him items from its news summary, and soon the messenger came to tell me that the messages were heard distinctly at the other end. The longest message which I sent was the following from that paper:—"The Americans of London have made arrangements to celebrate the coming Fourth of July," and the messenger brought me back from Sir William Thomson the exact repetition of the message. Thereupon we exchanged places, and I could not only hear distinctly the utterances of my colleague, but I could even distinguish the ictus of his voice. The results convinced both of us that Prof. Bell had made a wonderful discovery, and that its complete development would follow in the near future.

The news of these successful experiments soon circulated freely, and the day following, or possibly two days afterwards, Mr. Gray came to me and inquired whether the reports of our success with Bell's telephone were correct; and upon receiving from me an affirmative reply, he said that it was impossible, that we had been deceived in some way, that the transmission was by actual metallic contact through the wire, and that it was, to use his own words, "nothing more than the old lover's telegraph." In reply I said to him that we had taken every possible precaution against error, that we were both convinced of the reality of Bell's claims, and that Sir William Thomson would report to that effect. He persisted in his statement that the result was impossible, and that we must have been deceived in some way or other.

After having had direct knowledge of Mr. Gray's views at that time, I must confess to some astonishment at his claim now made that he anticipated Mr. Bell in the invention of the speaking telephone. Several months ago I saw an article in *Scribner's Magazine*, by Mr. Prescott, in which, while no direct assertion was made that Mr. Gray was the first inventor, there were illustrations given to show the development of the invention in chronological order, and Mr. Gray's instrument was there given priority. I had it in mind then to write a note to Mr. Prescott upon this subject, but I feared that there might be unpleasant controversies over the patents, and, the claim of Mr. Gray being rather indefinitely stated, I held my peace. But now that the error appears to be taking root, I have felt it to be my duty to make the statements above given. I have before me a letter from Mr. Bell, dated at Boston, Wednesday, June 28, 1876, and directed to me at Philadelphia, in which he gives diagrams showing how we might arrange the apparatus to transmit articulate speech, as he believed, from Boston to Philadelphia, and proposing experiments to that end if the judges should so desire.

In conclusion I ought to state further, that after Sir William Thomson's address at Glasgow had brought the telephone into

notoriety, Mr. Gray, whose instruments had also been called telephones, gave a public exhibition, in Chicago, I think, and in the report of his lecture which I read, he never once alluded to Bell's invention. His discourse was then, as at Philadelphia before the judges, solely in reference to the musical telephone. In fact, the newspapers had to take pains to inform the public that Mr. Gray's invention must not be confounded with Mr. Bell's, to which Sir William Thomson had referred. You will imagine, then, the surprise of the judges who examined these inventions particularly at Philadelphia in 1876, and heard the personal explanations made by the inventors, to be told now that Gray had already invented the speaking telephone, when all his statements then made show directly to the contrary.

Ann Arbor, November 18 JAMES C. WATSON

The Intra-Mercurial Planets

NATURE (vol. xviii. p. 569), in commenting upon my letter published the previous week, regarding the discovery of Vulcan, accused me of being not only "indefinite," but "contradictory." The number containing my letter (p. 539) has, from some unknown cause, not yet reached me, though I am in receipt of four numbers published later.

In the several articles written by me on that subject—to the Chicago Astronomical Society, to the Naval Observatory at Washington, to the Astronomer-Royal, to Admiral Muochez of the Paris Observatory, and to others—I have invariably stated the facts as they occurred under my observation, and as they impressed themselves upon my mind, and have invariably adhered to these statements, viz., that the two stars seen by me were of about the fifth magnitude, about 7′ or 8′ apart, with large red disks, and pointing towards the sun's centre. It is true my letter did contain an error, but not of observation, nor of estimation. In reducing the 8′ of arc (the estimated distance between the stars) to time, I somehow called it 2′, when, in reality, it is but 32s., thus not only changing its position in R.A., but also increasing, in this element, the discordance between Prof. Watson and myself. The detection of this error has changed, to me, the whole aspect of the Vulcan question. I had previously written to Prof. Watson that I could not reconcile his observations with my own either in R.A., or in Dec., but did not tell him what changes were necessary in order that they might harmonise. He gave me his corrected positions, which helped matters considerably, but still his R.A. was too great, and Dec. too little, for, from three estimations, the two stars ranged with the sun's centre. Recently I have been experimenting with a^1 and a^2 Capricorni (two stars which, in respect to distance from each other, resemble those I saw during the eclipse), my object being to test the accuracy of estimations made of the directions towards which two stars will range when hastily brought into the estimated centre of the field of a telescope having a diameter of one and a half degrees. I find that unless the objects are brought exactly to the centre, they do not point to the same place. During totality time was, of course, too precious to waste in being precise in this, and yet I endeavoured to be so, and as at each of the three comparisons they seemed to range with the sun's centre, I feel convinced that I was not far out in my estimated Dec.

In order to meet Prof. Watson's excessive R.A., I published (contrary, however, to my better judgment), that the distance between the stars was about 8′ instead of 7′ (as previously announced). On the assumption, therefore, that (*a*) one of the objects was θ Cancri, and (*b*) that they were 8′ apart, and (*c*) that the one nearest the sun was the planet, as Watson says, the position of the planet was as follows:—

Washington M. T.	
1878, July 29, 5h. 22 m. R.A., θ Cancri ...	8h. 24m. 40s.
Add 8′ =	32s.
Planet's R.A., Swift ...	8h. 25m. 12s.
,, ,, Watson ...	8h. 27m. 35s.
Difference	2m. 23s.
Dec. Swift	18° 30′
,, Watson	18° 16′
Difference	14′

It will be seen that there is a discrepancy between us of over a half degree of arc in R.A. If we saw the same objects how can we differ so widely? Could I be in error to the amount of 34′ between two stars in the same field? Can two stars be three and one half times the distance of Mizar from Alcor and an observer of experience estimate them at only 7′ or 8′? It will be remembered that I recorded in my note-book at the time the distance as 12′, but knowing how liable I might be to error in the valuation of so large a distance (for though, from practice, I can estimate quite closely double stars whose distances are from 2″ to 20″, I have had no experience in the estimation of those of several minutes separation), I chose to carry it in my mind until I should reach home, when it would be the work of only a few minutes to find two stars of the same apparent distance.

I said to Prof. Hough on our homeward journey, that, from memory, I thought their distance was about equal to that separating a^1 and a^2 Capricorni, and that I could decide when I should observe them. My memory of Mizar and Alcor was quite distinct, and as soon as I thought of those (which I did before my arrival at Kansas City) I mentally said, "A little over half the distance between them equals that between θ Cancri and the new object," which I did not doubt was Vulcan. Upon my arrival at home I immediately consulted "Webb's Celestial Objects," and was not a little surprised to find their whole distance to be less than 12′. Thus I know they were not over 8′ apart, I believe they were but 7′. I know they pointed to the sun's disk, I believe they did to his centre. I know they did not differ one-fourth of a magnitude in brightness, I believe they were exactly equal. I see them, in my mind's eye, as I then saw them, and, while consciousness endures, their image can never fade from the retina of my memory!

I consider the estimated distance in arc, made in such great haste, as valueless compared with the distance as impressed upon the mind from three comparisons, and verified by observations of a reliable character since arriving at home.

Can any error, then, be ascribed to the measurements of Watson, a skilful observer, with telescope well mounted, and with appliances for measuring, and who not only did measure the position of the new planet, but that of the sun and θ and δ Cancri (three objects in its immediate neigbourhood) as well.

Have we any right to call in question the accuracy of his circles in giving the position of the new object when they correctly gave the positions of the others?

Wherein, then, lies the discrepancy, and how can it be reconciled? Again, Watson says the planet was much brighter than θ, while the stars which I saw were of equal magnitude.

Several times since my return from the eclipse expedition I have, both in darkness and in strong twilight, examined θ, and I find no star near it, nor no two stars in its vicinity answering, in any particular, to those seen by me at Denver.

The above facts I submit to the world, and astronomers must deduce therefrom their own conclusions as to what the objects were. My own are reached, and, briefly stated, are as follows:—That the two objects seen by me were both intra-Mercurial planets, and that I did not—as was for a time supposed—see θ Cancri. Prof. Watson saw θ, and, some 42′ of arc south-east of it, another planet, and determined its position, and near to ζ Cancri still another, whose position also he fortunately ascertained, making four in all. It will not do to say, as some have intimated, that Watson saw θ Cancri, and 42′ from it a planet which I did not see, and that I, also, saw θ, and, 7′ or 8′ from it, another planet which he did not see. This reasoning appears to me untenable, for how could he have failed to see mine, when the diameter of his field was over 40′, and had θ in its centre?

If the above conclusions are true, and that four planets were discovered instead of two (as at first supposed), the question naturally arises, Which, if any one, is Lescarbault's Vulcan?

I estimated, at the time, the objects as being of the fifth magnitude, that is, as bright as a fifth magnitude star would appear in a clear, dark night. How much allowance ought to be made for diminution from atmospheric illumination I know not. I was then of the opinion that it would make a difference of at least one magnitude, but, having examined the region around θ, and finding many stars there, and several which are quite bright, not one of which I saw during the eclipse, I think that fully two magnitudes should be allowed.

In what way can these intra-Mercurial planets (of which there are probably many) be detected?

I would suggest that, on July 29 next, a determined and systematic effort be made, with large telescopes equatorially mounted, to observe θ Cancri, and, if then successful, there is hope that these planets, or some of the larger ones, may be discovered in the absence of a total eclipse, or while in transit. If

θ cannot be thus seen, then it appears to me that all time spent in their search in the sun's vicinity, except during a total or very large partial eclipse, would be time lost.

Rochester, N.Y., November 8 LEWIS SWIFT

Colour-Variation in Lizards.—Corsican Herpetology

IN a communication sent to you by my friend Mr. Wallace, under the title, "Remarkable Local Colour-variation in Lizards," published in NATURE, vol. xix. p. 4, mention is made of the well-known case of *Lacerta* (Podarcis) *muralis*, var. *faraglionensis*, only found on the Outer Faraglione of Capri, but there are many similar cases to my knowledge, and I add a note of them, for the fact, although unexplained, is one of great interest. During the last two years while engaged in forming a complete series of the Italian vertebrate animals, I have visited and explored most of the Mediterranean islands included in the Italian sub-region, and I have invariably found that our common lizard (*Podarcis muralis*) constantly presents dark varieties *on islets adjoining small islands*: this is the case on the Scuola, near Pianosa, on the Scoglio di Mezzogiorno, off Palmarola (Ponza), on S. Stefano, off Ventotene, on the Toro, off Vacca (Sardinia), on Lisca nera, Lisca bianca, and Bottaro, off Panaria (Lipari), on Filfla, off Malta, and on Linosa, near Lampedusa. The extreme cases are those of the Faraglione off Capri and Filfla, where a nearly intense black is obtained; next comes Toro, and next Linosa; only the latter case might be explained by the "struggle for existence" theory, for the lava rocks of Linosa *are* black; but such is certainly not the case with the other islets, and, *pace* Dr. Eimer, the Faraglione is gray, while Filfla—on which I spent a pleasant day in October last—is painfully white in the glaring Maltese sun, so that its black lizards are most conspicuous. I may add that few creatures I know vary more in colour than *Podarcis muralis*, even in the same locality; two most distinct varieties occur promiscuously on the small flat islet Formica di Grosseto.

Going over my Mediterranean herpetological notes reminds me of an interesting discovery I made last summer in Corsica, an island of great interest, which, strange to say, is rarely trodden by naturalists. Most of your zoological readers will be aware that, in 1839, Prof. Savi, of Pisa, described two new species of Italian Urodela, both from Corsica, viz., *Salamandra corsica* and *Megapterna montana*. The former has been quite neglected by modern herpetologists, or else placed among the synonyma of *S. maculosa*, simply because no one had Corsican specimens to compare. Now it is evidently nearly allied to the Continental form, but *quite distinct*, as the specimens I collected testify, all of them presenting the distinctive characters pointed out by Savi forty years ago. A nearly similar lot befell *Megapterna montana*, which Savi described nearly contemporaneously with Genè's description of *Euproctus Rusconii*, from Sardinia. Buonaparte, in his "Fauna Italica," united the two under the name of *Euproctus platycephalus*, given by Gravenhorst in 1829 to a newt, *sine patria*, preserved in the Breslau Museum; and most naturalists have followed Buonaparte, especially later writers on the subject, as Strauch, De Betta, and Schreiber, whilst others, acting more wisely, stuck to Genè's name. I believe that since Savi's day no one has studied the Corsican form, whose essential characters pointed out by the Pisan naturalist, who had only two specimens to work on, were overlooked even by his contemporary, the Prince of Canino; this explains all. Last year I rambled and collected all over Corsica, and found Savi's newt quite common in all the mountainous districts; I secured about 150 specimens of both sexes and all ages, even larvæ, and on my return to Florence was much surprised to find them *quite distinct* from the Sardinian *Euproctus* I possess; this made me refer to the original descriptions, and thus I found that Savi and Genè had described two very distinct species, and described them well. The two Italian species of *Euproctus* may be thus defined:—

E. Rusconii, Genè: Parotids wanting. Skin smooth, with small whitish tubercles scattered, especially about the sides of the head and neck. Female with a small conical pointed fibular tubercle, very like a rudimentary finger. Hind fingers slender and cylindrical. Irregular dark blotches on the throat. Size somewhat larger than the succeeding species. Hab. Sardinia.

E. montanus, Savi: Parotids small but distinct. Skin rough and granular. Female with a large, obtuse, compressed fibular tubercle, more like a ridge or crest, than anything else. Hind fingers stout, broad, and flattened. Throat uniform, rusty, without blotches; often a red or yellow dorsal stripe. Hab. Corsica.

As to *Euproctus platycephalus*, Gravenh., only a careful examination of the type-specimens, if yet existing in the Breslau Museum, can settle to which form it ought to be referred, but if their locality is unknown, I believe it better to suppress the name. *Euproctus platycephalus* is said to be found in Spain, but as I have no Spanish specimens, I cannot give any opinion on that form. In conclusion, I may add that Buonaparte was perfectly right in separating from the former the North African species *T. Poireti*, which is very distinct from our Italian *Euproctus*, in the shape of the head and body, and in the complete absence of any fibular tubercle in the female; it ought to be called *Glossoliga Poireti*. HENRY HILLYER GIGLIOLI

Florence, November 16

Commercial Crises and Sun-Spots

REFERRING to Prof. Stanley Jevons's article upon "Commercial Crises and Sun-spots" in NATURE, vol. xix. p. 33, I beg to draw your attention to the inclosed circular which I issued to my subscribers in April last.

The figures relating to the "Failures in England and Wales," were compiled by my clerks, under my own direction; those relating to the failures in the United States and Canada were supplied by Messrs. R. G. Dun and Co., of New York and London, and it may be observed how nearly they agree (*i.e.*, the failures in England and Wales, and those in the United States and Canada) in their fluctuations, and that there is an agreement between both sets of figures and the sun-spot period.

I have not been able to obtain similar figures for continental states, but I have observed that the complaints of depression in trade there agree, in substance and in time, with those in this country and North America. I have also noticed similar complaints from the southern hemisphere, especially New Zealand.

I refer to Dr. Hunter's suggestion of an Indian famine period in my circular, but I do not find that the famine period in India agrees, in point of time, with the depressions in the temperate zones; it is very probable that the excess of sunshine which produces drought and famine in India has an opposite effect on the prosperity of England and all other countries lying between the same isothermal lines, and that the more moderate degree of sunshine which may suit the Indian cultivator is insufficient to properly ripen English wheat and other produce (oats excepted).

Since April last I have taken several opportunities of ascertaining from agriculturists the effect of the variations in the sun-spots upon their yield of wheat, &c., and I find an agreement between them that during these years of minimum sun-spots the yield has proved bad when thrashed out, in consequence of the kernels being much smaller than in other years. I do not know whether the test has ever been tried or not; if not, I would suggest that some scientific observer should weigh an ounce, or a few ounces, of the kernels of each kind of grain grown in England every year, and count the number of them. I think it would be found that in years of maximum sun-spots wheat and barley kernels weigh their heaviest and oats their lightest, and that these proportions would be reversed in the years of minimum sun-spots. The difference in each kernel or in an ounce of them may, taken alone, appear trifling; but if it is an indication of the difference in the yield of the harvest throughout the whole kingdom, it may be a fact of the greatest importance as showing the cause of the cyclical variations in the prosperity of the country, and it may be of great value to land-owners and agriculturists generally as a guide in the rotation of crops and in allowing fields to lie fallow.

It is in this direction that I look for the causes of commercial depression. The whole of our "home" trade is dependent upon internal prosperity, and likewise a large proportion of our "foreign" trade. Other causes may have some effect upon either or both, such as peace or war, trade-unionism, bank-management, and the like; but the influence of the sun is too far-reaching and too powerful to be checked thereby. Man, by studying the working of its influence and power upon his daily life, may learn how to guard against much of the distress which periodically recurs. JOHN KEMP

Aspley Guise, November 16

"*London, April*, 1878

"*Failures in England and Wales*

"We append a Summary of the failures in England and

Wales, which it has been our duty to publish in *Kemp's Mercantile Gazette* during the past eleven years :—

Year.	1st quarter.	2nd quarter.	3rd quarter.	4th quarter.	Totals.
1867	3,981	4,081	3,555	4,233	15,850
1868	4,091	4,131	4,139	3,501	15,862
1869	3,819	3,997	3,495	5,207	16,518
1870	2,804	1,589	1,773	1,985	8,151
1871	2,142	2,191	1,837	1,994	8,164
1872	2,192	1,980	1,795	2,145	8,112
1873	2,354	2,299	2,054	2,357	9,064
1874	2,193	2,428	2,339	2,290	9,250
1875	2,331	2,277	2,133	2,453	9,194
1876	2,744	2,573	2,670	2,861	10,848
1877	2,829	2,856	2,610	2,952	11,247
Total for 11 years					122,260

. . . .

"The question occurs : Does the number of failures in a year depend upon natural causes? that is to say, Would the number rise and fall periodically according to the state of trade (or national prosperity) if the Bankruptcy Law remained constant? Whenever failures have become frequent, complaints have been made against the Law, and not without reason, but many who complain ignore the existence of any other cause. We compared the foregoing figures with the scientific tables recently published in NATURE, from the pen of Prof. Balfour Stewart,[1] and, being struck with the coincidence in their fluctuations, we further compared them with the statistics published by Messrs. Dun and Co., of New York, of the failures in the United States during the past eight years,[2] which period, being that of the existence of our present Bankruptcy Law, affords us a fair opportunity for making a comparison. Messrs. Dun and Co. report the following as the total failures in the United States during this period :—

1870,	Number of Failures,	3,551
1871,	,, ,,	2,915
1872,	,, ,,	4,069
1873,	,, ,,	5,183
1874,	,, ,,	5,830
1875,	,, ,,	7,740
1876,	,, ,,	9,092
1877,	,, ,,	8,822

"Evidently the same causes which were at work in England to depress trade and overwhelm the struggling and improvident classes, were equally effective in other countries—similar complaints of depression come to us from every part of the globe.

"The discussion which has arisen out of Dr. Hunter's suggestion of a 'famine period' in India, has brought to the public some knowledge of the existence of natural periods or cycles, of an average duration of 11·9 years each. The suggestion that England is affected with the same regularity is but reasonable, and although fortunately for us as a nation the effects do not produce famine, it appears evident that some degree of suffering is caused, and that the number of failures is thereby materially increased—the commercial panics which have occurred with about the same regularity furnish further evidence that this is the case.

"If we make due allowance for the excessive number in the last quarter of 1869, caused by the change in the Law, we find that the maximum number of failures in the last cycle occurred in the year 1868, which was the year succeeding the natural minimum ; hence we may conclude that about a year is required for the full effect of the natural depression to be reproduced in commerce. The twelve months from October 1, 1867, to September 30, 1868, appear to have been more serious to commercial men than either of the complete years, according to the number of failures :—

"In the 4th Quarter of	1867	there were	4,233 failures.
,, 1st ,,	1868	,,	4,091 ,,
,, 2nd ,,	1868	,,	4,131 ,,
,, 3rd ,,	1868	,,	4,139 ,,
Total			16,594

"These data indicate that we have not yet reached the worst of the present period—assuming that it runs an average length, we have to endure an increasing number of failures which will not reach its maximum until the fourth quarter of 1879."

JOHN KEMP AND CO.

Since this was written I have had counted the number of failures gazetted since January 1, 1878, and I find that they are *2,042* in *excess* of the corresponding period (January 1 to November 19) in the preceding year.

J. K.

[1] *Vide* NATURE, vol. xvi. pp. 9, 26, 45.

[2] Messrs. Dun and Co.'s Annual Circular, January, 1878.

Strange Properties of Matter

THE following are two experiments which will, perhaps interest some of your readers :—

Experiment No. 1.—The "Welding" of Metals at Low Temperatures

Some time ago, in order to estimate the amount of hydrocyanic acid in a solution, I precipitated it with silver nitrate. After having filtered and washed the precipitate, I reduced it to the metallic state by heating to the required temperature. Just as I was about to allow it to cool, I noticed a small piece of dirt among the reduced silver. In order to separate them, I took a thin platinum wire, and pushed the silver to one side, but on attempting to take the wire away the silver remained in contact with it. As I thought this curious, I tried the following experiment. I took a piece of silver foil about one centimetre square, placed it in an inverted porcelain crucible lid, and heated it to about 500° C. ; then I brought into contact with it the extremity of a thin platinum wire, and to my astonishment the wire raised the silver from the lid, and it remained in contact when cold, as the silver was so very much below its melting-point ; the above fact caused me some surprise, and I could not satisfactorily account for it.

I wrote to Sir W. Thomson, F.R.S., giving him a description of the above experiment, and in return I received a reply asking me to come and show him the experiment at his laboratory. I accordingly went up to the Glasgow University, and repeated it before him. He was very much interested, and advised me to write to NATURE, giving a description of the experiments. Sir W. Thomson gave the following explanation—That it was a remarkable case of "cohesion," the two metals, in fact, "welding," although the temperature was far below the melting-point of silver. The above experiment can be performed successfully at lower temperatures than 500° C., if smaller pieces of foil are taken. Other metals, for instance, copper and aluminium, cohere to silver in the same manner as platinum, but less strikingly.

Experiment No. 2.—A Curious Resonator

Some months ago I made the following experiment :—I took a small tuning-fork and struck it on the table. After the note had died away, so that it was no longer audible, I held the fork in the tip of the flame of a Bunsen burner, when the note was given out, so that it could be heard at some distance. I showed Sir W. Thomson this experiment, who gave the following explanation—That owing to the difference in density of the gases in the flame, the flame acted as a resonator, and so the note was emitted.

It seems to me that experiment No. 1 could be made the subject of an interesting research, but as I am wholly engaged in commercial pursuits, I am unable to take it up.

CHARLES A. FAWSITT

Glasgow, November 12

Galvanometer for Strong Currents

I MUST confess that I was surprised by Mr. R. E. Baynes' communication, in NATURE, vol. xix. p. 33, that the galvanometer I have proposed in NATURE, vol. xviii. p. 707, has already been described. Before writing my article I have searched a good many books and journals relating to the subject without finding an allusion to any such instrument. Since Mr. Baynes drew my attention to "The Elements of Physical Manipulation," by Prof. Pickering, of the Massachusetts Institute of Technology, U.S., I have procured this book and find that it certainly does contain the theory of a galvanometer like mine, with the coil moving round a horizontal axis. As far as I know, such an instrument has, however, not been practically employed either in this country or on the Continent before I introduced it, though its want must have been much felt for some time past. This seems to show that Prof. Pickering's description of the instrument has not been brought to the general

knowledge of electricians. If, therefore, my article in NATURE, at a time when electric currents of great strength are being so widely introduced into practical working, has contributed to make electricians acquainted with this form of galvanometer, I shall not regret the time I have spent in theoretically and experimentally investigating this subject. Prof. Pickering alludes in his book to the improper *dip motions* of the needle as a defect in his form of galvanometer; I think I have obviated this by pivoting the axis of the needle at both ends. Since communicating the results obtained with my *experimental* instrument, more elaborate instruments have been constructed and found very useful in many instances where the ordinary forms of galvanometers would not have answered the purpose.

EUGEN OBACH

Woolwich, November 19

Utilisation of the African Elephant

I have just read some remarks in NATURE (vol. xix. p. 54) on the utilisation of the African elephant, which I think are worth considering. Judging from the specimens in the Regent's Park Gardens, which I suppose have not been selected in any way, and are therefore only average samples of the African species, I should say that the African elephant would prove hardier and capable of more work than the general run of Indian elephants. I was surprised to find that the two African elephants I saw in the Regent's Park were what the mahouts call "Dohara Band," which I would translate "double constitution." I do not think that more than five per cent. of Indian elephants are placed in this class. It is very rare indeed to see an elephant of this class in a Government Feelkanah, or in the hands of any European, because wealthy natives value them so highly and give such enormous prices for them, that even when caught by a Government keddah officer they are often sold on account of the fancy prices they fetch The "Dohara Band" elephant will do a wonderful amount of work on a small quantity of food, and stands fatigue and exposure to the sun far better than any other sort.

To commence elephant-catching operations in Africa, six "koonkies," *i.e.*, elephants trained for catching purposes, would be required, and about twenty men from Assam or some other jungle district of Bengal would be sufficient. With this establishment it would be possible to catch and train at least two hundred elephants in the course of twelve months.

H. L. JENKINS

Clanacombe, Kingsbridge, South Devon, November 24

OUR ASTRONOMICAL COLUMN

ORBITS OF BINARY STARS.—In a communication to *The Observatory*, Dr. Doberck, of Col. Cooper's Observatory, Markree Castle, has summarised the results of the investigations on the orbits of the revolving double stars which have occupied him between three and four years, and which he has conducted with so much skill and laborious application. In a climate where the skies are too irregularly favourable to allow of an astronomer occupying himself wholly upon observations, it would be difficult to name any more interesting work to which he could devote his leisure, than such a revision of the elements of the binary systems. Dr. Doberck's account of his methods in different cases will be of much service to any one who may engage upon similar researches. He has found no reason to detract from the value of the early, graphical method of Sir John Herschel; on the contrary, instances are mentioned where it has been of the greatest service in tracing out the general form of the orbit, for correction by more refined processes, as in the difficult case of Σ 1768; indeed, Dr. Doberck considers it superior to the second method given by Herschel from its admitting of the weights being approximately taken into account with ease. In a provisional orbit for the close double star Σ 3121, the period assigned is thirty-seven years; at present we know of only two more rapid binaries. Some of the orbits included in Dr. Doberck's paper, have been made the subject of communications to the Royal Irish Academy, and have been published in the *Transactions*.

LALANDE'S STARS, NOS. 5,499 AND 45,400.—Mr. J. E. Gore writes suggesting variation in both these objects. In examining the question of variability of any of Lalande's stars, Mr. Gore will find it necessary, in the first instance, to have recourse to the original observations as printed in the "Histoire Céleste," there being many errors in the reduced catalogue. Thus, No. 5,499 appears through a misprint at p. 246, where the transit at the third wire is given as 2h 47m. 45·2s., instead of 2h. 42m. 45·2s. The star is really No. 788 of Weisse's Bessel. The following star is also thrown out by a similar error in the time of transit, so that Lalande, 5,520, requires a correction of – 5m. in R.A. With regard to No. 45,400, the suspicion of variability is probably occasioned by a misprint in the "Histoire Céleste," since Piazzi, Bessel, and others estimate the magnitude the same as at present.

THE ANNULAR ECLIPSE OF JANUARY 22, 1879.—The first of the annular eclipses of the ensuing year, a return of that of January 10, 1861, which was central in Australia, commences in Uruguay, whence the belt of annular phase traverses the South Atlantic, passing over Tristan d'Acunha, the few inhabitants of which islands may probably be startled by seeing the sun transformed into a narrow luminous ring while he is high in their heavens. The central line crosses the African continent in the direction of Pemba Island, north of Zanzibar, where the annularity will continue nearly three minutes: the middle of the eclipse at 4h. 6m. P.M., local mean time.

GEOGRAPHICAL NOTES

THE Council of the Royal Geographical Society have determined to commence, on January 1, 1879, the monthly issue of a new series of their *Proceedings*, under the title of *The Proceedings of the Royal Geographical Society and Monthly Record of Geography*. The latter part of this title will, we believe, fairly indicate the nature of the contents, which will include the papers read at the evening meetings, original articles, geographical notes, obituary notices, proceedings of geographical societies, and brief analytical notes on new books and maps. Each monthly number will be illustrated with one or more maps, and no doubt in this respect endeavours will be made to meet the wants of a public larger than that contained within the ranks of this popular society, which now numbers some 3,400 members.

IN connection with this we regret to announce that this month's number of the *Geographical Magazine* brings the career of that journal to a close. This regret, however, is considerably mitigated by the fact that the new form of the Geographical Society's organ is intended to take the place of the journal which for so many years has been so ably conducted by Mr. C. R. Markham. Mr. Markham deserves great credit for his disinterestedness in continuing to carry on a journal which aimed so successfully to be the organ of scientific geography in this country. The volumes will form a valuable record of the progress of geography for the period over which they extend. In the December number Mr. Markham gives an account of the career of the journal since its first start as *Ocean Highways* in 1870. We trust that the new organ will prove a worthy successor of its predecessor, and that while giving due prominence to geographical news, the conductors will aim at bringing geography under the guidance of sound scientific principles.

THE Church Missionary Society have recently received from Mr. A. M. Mackay, of their Nyanza Expedition, the journal of his experiences, extending from December 31, 1877, to May 16, 1878, in which occur some useful suggestions to African travellers, as well as information of considerable interest to geographers. On April 30 Mr. Mackay reached Uyui, after a hurried and tedious tramp

of 350 miles, undertaken with the view of aiding the Rev. C. T. Wilson, and he promises a detailed account of his troubles then on another occasion. In the course of this march he had to cross the extensive wilderness of Mgunda Mkali, which he describes as being for the most part not merely a swamp, but this year more under water than above it. Day after day the party waded and splashed through mud and water, now over the ankles, sometimes up to the knee, and here and there up to the waist or higher. Hopeless and still more hopeless, Mr. Mackay remarks, the wading seemed to become, and he found that they were crossing what was neither more nor less than the source at once of the Nile, the Congo, and the Rufiji. This gigantic boggy plain or moss Cameron calls the Nya Kun Swamp, and, where Mr. Mackay crossed it, in S. lat. 5° 20′, his aneroids recorded an average elevation of 4,000 feet exactly. To the north, in Usukuma, the swamp narrows itself into the sluggish Lewumberi River, the most southerly of the sources of the Nile. From this swamp, too, the rapid Mdaburu River takes its rise, and flows southward into the Ruaha, in Unyoro, and Uhehe, and thence to the Indian Ocean, where it is known as the Rufiji. A day's march east of Tura Mr. Mackay found the swamp contract to a breadth of 300 yards, with increased depth, and slight indications of flow to the southward; it then rounds to the west, steering clear of Iiwe-la-Singa, and, after two more days, it was crossed—a flooded, five-armed river, flowing rapidly north at an altitude of 3,700 feet. Here it is called the Nghwàla River by the Wanyamwezi, from the number of partridges on its banks. In Speke's map alone it would seem that the true course of the river is indicated; it flows north-east to Mirambo's country, where it is known as the Ngombe (*i.e.*, ox) River, and finds its way into the Malagarasi, thus aiding that river in bringing Lake Tanganyika nearer to permanent overflowing, when the Lukuga—which has been such a bone of contention to two great travellers—will no longer be a swamp but a decided stream, and the waterline of the Congo will run from Loango on the West Coast to the confines of Ugogo.

THE London Missionary Society have received a letter from the Rev. J. B. Thomson, dated from Ujiji, announcing the safe arrival on August 23 of the main body of the Society's expedition at its destination on Lake Tanganyika. Though, as will be remembered, this expedition met with a long series of disasters and delays in the coast region, they have now been successful in performing one of the quickest and most prosperous journeys from Mpwapwa to Ujiji, having been but seventy-three days on the road. Messrs. Thomson and Hore have already found an apparently healthy site for their station close to Kinegoma Bay, and about three miles from Ujiji.

WE learn from a Japan paper that an American gentleman has been engaged for some time past in surveying the Island of Yezo, as well as in making geological investigations. The result is said to be that there are 7,000 square miles of land fit for agricultural purposes, and 6,000 suitable for pasture, while there are 5,000 square miles of forests and 9,000 of volcanic mountains and mineral country. An impression appears to prevail that the Government wish to encourage emigration to this thinly-populated part of the Japanese empire.

As an erroneous impression prevails that nothing is known of the scientific work done by the Portuguese African expedition previous to the date mentioned in last week's NATURE, it may be interesting to give, as instances of the service which they are rendering to geography, the positions of some of the places determined by Senhor Serpa Pinto and his colleagues:—Benguela, long. 13° 25′ 20″ 45‴, lat. 12° 34′ 17″, alt. 7 metres; Dombe Grande, long. 13° 7′ 45″, lat. 12° 55″ 12′, alt. 98 metres; Quillenques, long. 14° 5′ 3″, lat. 14° 3′ 10″, alt. 900 metres; Caconda, long. 15° 1′ 51″, lat. 13° 0′ 44″, alt. 1,678 metres; Bihé, long. 16° 49′ 24″, lat. 12° 22′ 40″, alt. 1,670 metres. The longitudes are stated to be chronometrical.

DR. EDWIN R. HEATH, of Wisconsin, is about undertaking the exploration of the Beni and Madre di Dios Rivers of Brazil, his sojourn in South America some years as secretary of legation in Chili giving him excellent advantages for this purpose. Dr. Heath had arranged to visit South America with Prof. Orton, but was detained, and he now desires to carry out some of the work that the untimely death of that well-known explorer has left uncompleted.

THE latest advices from Mr. Frederick A. Ober, of whose explorations in the West Indies on account of the Smithsonian Institution we have given notice from time to time, were from Point à Pitre, Guadeloupe, on September 23. He was about to leave for the United States, expecting to arrive some time between the middle and end of October. Since his last report he has obtained quite a number of additional collections, and hoped to complete the material for the proposed catalogue of the birds of the West Indies. So far the collections sent forward by him to the Smithsonian Institution have been found to contain some seventeen undescribed species of birds, as determined by Mr. George N. Lawrence, of New York.

AN interesting account of a recent visit to Pitcairn Island by Admiral De Horsey in the *Shah*, forwarded to the Admiralty, will be found in yesterday's *Daily News*. The people are evidently as primitive and well-conducted, and on the whole as comfortable as ever.

IN a previous number we referred to a work of great geographical interest—"Die Sahara, von Oase zu Oase," by Dr. Joseph Chavanne, published by Hartleben, of Vienna. At that time the work was in course of publication, and we refer to it now to announce its completion in twenty parts. The last parts are in every way equal to the earlier ones, and if anything the interest is rather increased than otherwise. The work contains numerous woodcuts, besides seven coloured plates and a map of the great desert; its perusal will be found extremely attractive by any one taking interest in geographical science. The exact route which Dr. Chavanne describes is the following:—The travellers start from Tripolis through the Fezzan to Mursuk, then westward to Rhat, the land of the Tuareg or Imoshag. From Rhat they turn northward to Rhadames, thence to Biskra in Algeria. Here the travellers again turn their backs to the Mediterranean and proceed in a south-westerly direction by way of El Aruat and El Golea to Insalah. From Insalah they go to Tafilet, in the extreme north-west of the desert, and thence many thousand miles to the south to Timbuctoo. The Oasis of Air or Asben is the next station, situated due east from Timbuctoo, then Tibesti, the land of the Tebbu. Thence they turn to the north-east to the Jupiter Ammon Oasis, which is the furthest point to the east reached. The travellers then turn westward again and return to Tripolis by way of Audschila.

ON THE DEVELOPMENT OF THE GARPIKE

THE gar or bony pike of North America is one of the most interesting of living fishes. The best known species of the genus to which it belongs is the *Lepidosteus osseus*. This species owes the grammatical form of its scientific name, and, indeed, its first scientific description to the elder Agassiz, and we have now to record the filling up of the last details of its life-history to the younger Agassiz. Known for over three-quarters of a century, it has been only within the last few months that the young fish as they escape from the egg have been

seen, and it has been the good fortune of Alexander Agassiz to succeed in hatching the eggs and raising the young until they showed at least the principal structural peculiarities of the adult. A short account of the chief facts in connection with this stage of the bony pike's history will appear in the forthcoming number of the *Proceedings* of the American Academy of Arts and Sciences; from an advance copy we cull the following details:—The spawning-ground selected for observation was the Black Lake, at Ogdensburgh, N.Y. Mr. Garman, who describes the scene, and Mr. Blodgett, who rendered most essential assistance, deserve the thanks of every naturalist. The eggs collected were carried by the hand in pails from Ogdensburgh to Cambridge, where their progress was watched by Prof. A. Agassiz.

The fish began to spawn about May 18. Little projections of granite stand out here and there into the lake. The frosts from time to time have broken off from these, small angular blocks, which lie piled together under the water at depths varying from two to fourteen inches. Into these shallows the female fish would come, each of them attended by two males. While very timid when in deep water, they seemed to be courageous to recklessness when they approached the shallows. On they would come in threes, when rising to the surface of the water, and thrusting their bill out of it they would open this widely, then take in air, and close it with a snap. In some few cases three or four males would be in attendance on one female, but much more often there would be but two, and these would swim resting on either side of the female fish, their bills reaching up toward the back of her head. At times the water would be lashed into all directions with their conjoined convulsive movements. The eggs when laid were excessively sticky; to whatever they happened to touch they stuck, and so tenaciously, that it was next to impossible to release them without tearing away a portion of their envelopes. It is remarkable that, as far as could be seen, there was, on and about the spawning ground, a complete absence of anything that might serve as food for the young fish.

Of the quantity of eggs brought to Cambridge, only thirty hatched, and not one of those artificially fecundated was hatched. In Prof. A. Agassiz' anxiety not to spoil this interesting experiment he did not venture to examine any of the fresh eggs; so that the history of their segmentation and very early development remains to be worked out. The envelope of the eggs is very opaque and of a yellowish green, like that of toads. Of the thirty hatched out by the end of May, twenty-eight were alive in the middle of July last. When first hatched the young fish possesses a gigantic yolk-bag, and the posterior part of the body presented nothing specially different from the general appearance of any ordinary bony (teleostean) fish of the same age; but the anterior part was most extraordinary: it looked like a huge mouth cavity, extending nearly to the gill opening, and crowned by a depression like a horse's hoof in outline, along the margin of which were a row of protuberances acting as suckers. The moment the young fish was hatched it attached itself to the sides of the vessel by means of these, and would hang immovable. The eye was not very advanced, the body was transparent, the gill covers were pressed against the sides of the body; the tail was slightly rounded, the embryonic fin is narrow, and there were no traces of embryonic fin rays; the olfactory lobes were greatly developed and elongated as in sharks and skates; the chorda was straight. On the third day the body became covered with minute black pigment cells, and then was noted the first traces of the pectoral fins, and the snout became more elongated; the great yolk-bag was greatly reduced in size. About the fifth day were seen traces of the caudal, dorsal, and anal fins. Gradually the snout became elongated, the suckers concentrated, and the disproportionate size of the sucking disc became reduced, so that when about three weeks old it became altogether more fish-like. The sucking disc was now reduced to a swelling at the top of the upper jaw, the yolk-bag had disappeared, the gill covers extended well up to the base of the pectorals—these latter were in constant motion, and the tail exhibited the same rapid vibratile movements. The young fish now begins to swim about, and is not so dependent upon its sucking disc, and at last this only remains as a fleshy globular termination on the snout. At this stage, too, the young have the peculiar habit of the adult fish of coming to the surface to swallow air. When they go through the process under water of expiring this air they open their jaws wide and spread their gill-cover, and swallow as if they were choking, making violent efforts, until a minute bubble of air has become liberated, when they become quiet again. Their growth is rapid. Within a month the teeth made their appearance, and some of the fin-rays on the fringe of the pectorals were to be seen.

Prof. A. Agassiz draws the following conclusions from these observations:—"That notwithstanding its similarity in certain stages of its growth to the sturgeon, notwithstanding its affinity with sharks by the formation of its pectorals from a lateral fold, as well as by the mode of growth of the gill openings and gill arches, the Lepidosteus is not at all so far removed as is generally supposed from the bony fishes." The memoir is illustrated by five plates containing some forty-five figures, and is only to be regarded as a preliminary account, but it is a preliminary account of such exactness, importance, and interest, that no apology is necessary for bringing it at once under the notice of our readers. This memoir was presented to the American Academy as recently as October 8 last. E. PERCEVAL WRIGHT

THE MUSIC OF COLOUR AND MOTION

AT the Physical Society, on November 23, 1878, Prof. W. E. Ayrton, late of the Imperial Engineering College, Tokio, Japan, read a paper, written by himself and Prof. J. Perry, of the same college, on "The Music of Colour and of Visible Motion." The authors began by pointing out the well-known fact that emotion is excited by moving bodies, and they believed that, upon this basis, a new emotional art would be created which would receive a high development in the far distant future. All methods of exciting emotion could be cultivated; but of these, music, by reason of the facility with which its effects could be produced, had alone been highly perfected by the bulk of mankind. Sculpture and painting are not purely emotional arts, like music, inasmuch as they involve thought. It would take a long time and much culture for the eye to behold moving figures with similar emotional results to those of the ear on hearing sweet sounds; but time and culture only might be necessary. It might be due to their neglect of this emotional tendency that the Western nations felt little emotion at moving visual displays. For among the Eastern nations they had entertainments consisting of motions and dumb show, which, although incomprehensible and even ludicrous to the European, powerfully affected the feelings of a native audience. In Japan the authors had seen whole operas of "melodious motion" performed in the theatres, the emotions being expressed by movements of the body, affecting to the audience, which were quite strange to them. The accompanying orchestral music was, withal, displeasing to the authors, while, on the other hand, Western music is mostly displeasing to the Japanese.

The emotions produced by rapidly-moving masses, such as a train bowling up to a bridge, or by changing colours, as in sunsets, have been felt by all, and those excited when the moving bodies are very large do not

seem to be producible by anything else in nature. Harmonic instruments have been constructed to exhibit the combination of two or more pairs of harmonic motions to the eye; for example, Blackburn's pendulum, Lissajous' forks, Wheatstone's kaleidophone, Yeates' vibrating prisms, Donkin's and Tisley's harmonographs, and Hopkins' electric diapason. Prof. Ayrton illustrated his remarks by exhibiting these instruments in action. The pendulum traced out the complex path of the combined motions by a jet of falling sand, the forks or prisms by a moving beam of light thrown on a screen, the kaleidophone by a bright bead, and the harmonograph by the involutions of an aniline pen. With none of these and such like instruments, however, is the production of mere emotion the end in view; and in some of them no change can be made in the periods of the pairs of harmonic or periodic motions combined without arresting the instrument, a proceeding which in music would be analogous to stopping the tune at the end of every chord. There is no provision either for changing the amplitude or phase, equivalent in music to an inability to render, at will, a note forte or piano, or rather as it is not only the strength of the entire note, but even the amplitude of the various component harmonics that these instruments cannot regulate, it would be as if in music there was the probability of a note marked in the score as piano for the flute being rendered by a loud blast from a trumpet. A successful instrument in the new kinematical art must at least visibly render changes in period, amplitude, and phase of the harmonic motions represented. Profs. Perry and Ayrton had designed an instrument, which is now in Japan, for effecting these required changes in a combination of harmonic motions given to a moving body, and which they claimed to be the first musical instrument of the visual art in question. They had not given it a name yet, because the nomenclature of the subject was uninvented. Photographs and diagrams of this instrument were exhibited to the meeting. It consists of a mechanical arrangement of sliders, pulleys, and cords, whereby two motions, one along a vertical, and the other along a horizontal line, and *each* consisting of the *sum* of a number of harmonic motions the *period*, *amplitude*, or *phase* of *any one* of which can be varied at will, are compounded in the resultant motion of a suspended pane of glass. A black circle painted on the pane is intended to represent the moving body as projected against a wall or screen behind. The sliders controlling the motion of the pane are actuated by a revolving barrel, the periphery of which is carved according to mathematical principles, so as to give the different harmonic motions to the sliders in one revolution. The motion is further regulated by shifting the sliders either parallel to the axis or at right angles to the radius of the revolving barrel; and by the angular velocity of the barrel. In this way the period, amplitude, and phase of the component motions of the glass either in a vertical or horizontal direction, may be changed at will, and almost immediately. Other kinds of periodic motions may be compounded in a similar way. Prof. Ayrton also suggested other forms of apparatus for this purpose. Numberless combinations of graceful motions producing emotional effects on the beholder can by its means be given to a visible body. It is the intention of the authors to construct an improved form of the apparatus, and to arrange for the blending of colour with the moving body to heighten the emotional influence; for example, they purpose having changing mosaics of different hues, thrown upon the screen for a background to the black spot. This can be done by means of an instrument similar to the chromotrope with its revolving sheets of parti-coloured glass. In conclusion Prof. Ayrton said that there might yet be invented many different ways of producing these spectacles, and there was no reason why a whole city full of people should not enjoy these displays projected upon the clouds overhead.

THE SWEDISH NORTH-EAST PASSAGE EXPEDITION

FROM letters despatched from the mouth of the Lena by Prof. Nordenskjöld on August 27, which have just been published in the Gothenburg *Handels Tidning*, we learn that the *Vega* accompanied by the *Lena* left Dickson Harbour, at the mouth of the Yenissej, on August 10, the weather being fine. On the 11th ice was seen, but it consisted almost exclusively of bay ice which did not obstruct navigation, which, however, was rendered difficult by a thick fog. The salinity of the water began gradually to increase and its temperature to fall. Organic life at the bottom grew richer at the same time, so that Dr. Stuxberg on the night between August 13 and 14, while the vessel lay anchored to a drift-ice floe, collected with the swab a large number of beautiful pure marine types; for example, large specimens of the remarkable crinoid, *Alecto eschrichtii*, numerous asterids (*Asterias linckii* and *panopla*), pycnogonids, &c. The dredgings near the land now too began to yield to Dr. Kjellman several large marine algæ. On the other hand the higher plant and animal life on land was still so poor that the coast here forms a complete desert in comparison with the rocky shores of Spitzbergen or West Novaya Zemlya. Auks, rotges, loons, and terns, which are met with on Spitzbergen in thousands upon thousands, are here almost completely absent. Gulls and *Lestris* which there fill the air with continual sound occur here only sparingly, each with two species, and it appears as if they quarrelled less with one another. Only the snow-bunting, six or seven species of waders, and a few varieties of geese are found on land in any great numbers. If we add a ptarmigan or two, a snowy owl, and a species of falcon, we have enumerated the whole bird fauna of the region, at least so far as the Swedish expedition have been able to ascertain it. Of warm-blooded animals in the neighbouring sea, only two walruses and some seals, *Phoca barbata* and *hispida*, were met with. There is probably great abundance of fish. Cosmic dust was sought for on the ice without success, but there was found upon it some yellow specks which, on examination, were found to be a coarse-grained sand, consisting exclusively of very beautifully-formed crystals up to two millimetres in diameter. The nature of these crystals was not ascertained, but it was evident that they are not formed of any ordinary terrestrial mineral, but possibly of some substance crystallised out of the sea-water during the severe cold of winter.

The *Vega* lay at anchor from August 14 to 18 in a harbour named Actinia Harbour, from the number of these animals brought up by the dredge from the sea-bottom. This harbour is situated in a sound between Taimyr Island and the mainland. The land was free of snow, and covered with a greyish-green turf formed of a close mixture of grasses, mosses, and lichens, forming a reindeer pasture much superior to that of the valleys in Spitzbergen which abound in reindeer. Only a few reindeer, however, were seen here, probably owing to the presence of wolves. The number of phanerogamous plants is exceedingly small; the moss and especially the lichen vegetation, on the other hand, abundant enough. Actinia Harbour is an excellent position for a meteorological station. The fog still continuing, the *Vega* and the *Lena* sailed again on the 18th, and reached Cape Chelyuskin on the 19th, anchoring in a little bay which indents the low promontory, dividing it into two parts. The western point was found to be situated in 77° 36′ 37″ N. L., and 103° 25½′ E. from Greenwich, and the eastern in 77° 41′ N. L., and 104° 1′ E. L. Inland the mountains appeared to rise by degrees to a height of 1,000 feet. These mountains, as well as the plains, were free of snow. Only here and there were to be seen large white patches of snow in hollows on the mountain sides or in some small depression on the plains. At the beach, however, the ice-foot still remained at most places.

The soil of the plains is clayey, partly bare, and cracked into more or less regular hexagonal figures, partly covered by a turf of grass, mosses, and lichens resembling that at the last landing-place. The rock here, however, was not granite, but upright unfossiliferous schistose strata rich in crystals of sulphide of iron, and crossed at the extremity of the cape by thick quartz veins. Dr. Kjellman could not find here more than twenty-four species of phanerogamous plants, most of them distinguished by a disposition to form compact half-globular tufts. The lichen vegetation was also, according to Dr. Almquist, monotonous, though luxuriantly developed. It almost appeared as if the plants of the Chelyuskin peninsula had attempted to wander farther toward the north, but halted when they met the sea, at the very outermost point. For here were found within a very limited space nearly all the plants, both phanerogams and cryptogams, which the land had to offer, and many of them were sought for in vain farther up on the plains.

Animal life on land rivalled the higher plant life in poverty. Of birds there were seen only a number of *Phalaropus*, some species of *Tringa*, a *Colymbus arcticus*, a very numerous flock of *Anser bernicla*, a few eiders, and the remains of a snowy owl. In the neighbouring sea, which was almost free of ice, were seen a single walrus, two shoals of white whales and some few seals (*Phoca hispida*). It, too, was here evidently very poor in warm-blooded animals. On the other hand the dredge brought up from the sea-bottom various large algæ (*Laminaria agardhi*, &c.), and a large number of lower animals, among them very large specimens of *Idothea entomon*, an isopod, which also occurs in the Baltic and the large Swedish lakes, and is looked upon as an evidence that during the ice age they were connected with the Polar Sea. The algæ obtained were interesting as affording further proof of the incorrectness of the view which long prevailed, that the Siberian Polar Sea was quite devoid of the higher algæ.

On August 20 the voyage was resumed, the course being set east by south, in the hope of falling in with a continuation of the new Siberian Islands. Drift-ice was soon met with, and by the morning of the 23rd it was found impossible to proceed further in that direction. An attempt was now made by sailing in a northerly and north-westerly direction to get out of the ice-field, and in about twenty-four hours the *Vega* was again in open water, and the same day land was sighted. The land was found to be the north-eastern extremity of the eastern Taimyr Peninsula, lying in about 76° 30′ N. L., and about 113° E. of Greenwich. The sea was completely free of ice for a distance of 15′ or 16′. Fine mountains 2,000 to 3,000 feet high were seen some distance inland. These, like the plains below, were free of snow to their highest summits. Some small glaciers were believed to be seen, but they ended at a height of about 800 to 1,000 feet above the sea.

Animal life now began to be very abundant. Dr. Stuxberg had, while the vessel lay anchored to a floe in the drift-ice-field, brought up from a depth of 35 fathoms an unexpected variety of fine marine animal types, among them three specimens of a crinoid supported on a stalk, probably young individuals of *Alecto eschrichtii*, which also was found in innumerable full-grown specimens, masses of asterids (for instance *Solaster papposus, endeca, furcifer, Pteraster militaris, Asterophyton eucnemis*), and of the otherwise exceedingly rare *Molpadia borealis*, a colossal pycnogonid of 180 millimetres diameter, &c. Not less abundant was the lower animal life at a smaller depth though the forms were partly different. The animals occurring here were evidently pure Polar Sea types without any immigration whatever from southern seas, such as has doubtless taken place in the case of the fauna of Spitzbergen.

On August 24 land was sighted, which was found to be Preobraschenski Island, near the mouth of the river Katanga. From this point to the mouth of the Lena the depth was only from 5 to 8 fathoms.

To judge from the experience of the voyage there is no more ice on the Siberian coast during the latter part of summer than in the White Sea during midsummer. Besides the ordinary observations of the temperature of the sea-water at the surface in connection with the common meteorological observations made six times in the twenty-four hours, the temperature and salinity of the water at different depths were determined two or three times a day. When the depth amounts to at least 30 metres the temperature at the bottom is found to vary between $-1{\cdot}0°$ and $-1{\cdot}4°$ C. The specific gravity of the water amounts there to 1·026—1·027, the salinity being little less than that of the water of the Atlantic Ocean. On the surface the temperature is exceedingly variable:—At Dickson Harbour $+10°$, a little south of Taimyr Sound $+5{\cdot}4°$, among the drift ice off that sound $+0{\cdot}8°$, off Taimyr Bay $+3{\cdot}0°$, at Cape Chelyuskin $+0{\cdot}1°$, off Katanga Bay $+4{\cdot}0°$, between Katanga and Lena $+1{\cdot}2°$ to $+5{\cdot}8°$. The specific gravity of the surface-water in a broad channel along the coast never exceeded 1·023, in general only amounted to 1·01 or under. The latter figure corresponds to a mixture of about one part sea water with two parts river water. This shows indisputably that a warm surface current of little salinity from the mouths of the Obi and the Yenissei runs first along the coast towards the north-east, and then under the influence of the rotation of the earth in an easterly direction. Other similar currents originate from the Katanga, Anabor, Olonek, Lena, Jana, Indigirka, and Kolyma, all which pour their waters, more or less heated during the hot summer of Siberia, into the Polar Sea and render it, during a short period of the year, almost free of ice.

On the night between August 27 and 28 the *Vega* parted from the *Lena* off the mouth of the River Lena. There is scarcely any hope now that the voyage will be completed before next summer. No doubt the *Vega* has got into a safe winter-harbour, and that during the detention of the expedition a rich harvest of scientific results will be gathered.

THE FORMATION OF MOUNTAINS

PROF. ALPHONSE FAVRE, of Geneva, has been making an interesting series of experiments to illustrate the formation of the great inequalities of the earth's surface by means of lateral thrust or crushing. These he describes and illustrates in a recent number of *La Nature*, to which we are indebted for the illustrations which accompany this article. Prof. Favre refers to the early experiments of Sir James Hall with various kinds of cloth, which he made to assume a variety of shapes by means of weights. He speaks of the various theories of the elevation of mountains, and especially of that of H. B. de Saussure, whose term *refoulement* seems to have meant much the same as that used by M. Favre, *écrasement latéral*.

The three systems, M. Favre says, which account for the origin of mountains by forces which push the great mineral masses from below upwards, from above downwards, or laterally, do not differ so much from each other as at first sight appears. Those geologists who have admitted the system of elevations as the principal cause of modification of the surface of the globe, would probably enough admit the formation of depressions as a secondary modification; and so those who have accounted for these modifications mainly by depression, would probably enough also admit elevation as a secondary factor. Again, in the system of lateral crushing, there is a general depression of the surface of the earth, since there is a diminu-

tion in the length of the radius of our globe, and yet there result elevations of the ground in the midst of this general depression.

The cause of lateral crushing, M. Favre goes on to say, is owing to the cooling of the earth. It is, in fact, very probable that our globe is at the stage when, according to Élie de Beaumont, "the mean annual cooling of the mass exceeds that of the surface, and exceeds it more and more." It must follow that the external strata of the globe, tending always to rest on the internal parts, are wrinkled, folded, dislocated, depressed at certain points, and elevated at others.

"The experiments," M. Favre continues, "which I have made at the works of the Geneva Society for the manufacture of physical instruments, resemble much those of Sir James Hall; they differ notably, however, in two points:—1. The celebrated Scotchman caused the matter which he wished to compress to rest on a body which itself could not be compressed, while I placed the layer of clay employed in these experiments on a sheet of caoutchouc, tightly stretched, to which I made it adhere as much as possible; then I allowed the caoutchouc to resume its original dimensions. By its contraction the caoutchouc would act equally on all points of the lower part of the clay, and more or less on all the mass in the direction of the lateral thrust. 2. Hall compressed, by a weight, the upper surface of the body which he wished to wrinkle, which prevented any deformation, while by leaving that surface free, I have seen, during the experiment, forms appear similar to those of hills and mountains which may be observed in various countries."

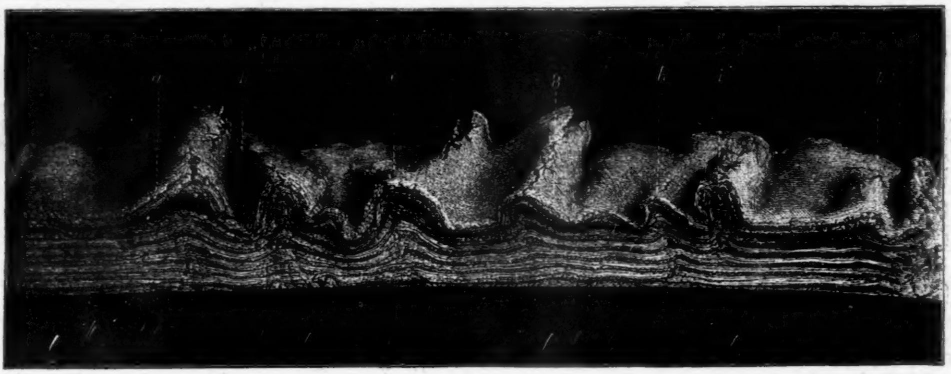

FIG. 1.

"The arrangement of the apparatus is very simple. A sheet of india-rubber 16 mm. in thickness, 12 cm. broad, and 40 cm. long, was stretched, in most of the experiments, to a length of 60 cm. This was covered with a layer of potter's clay in a pasty condition, the thickness of which varied, according to the experiments, from 25 to 60 mm. It will be seen from the dimensions indicated that pressure would diminish the length of the band of clay by one-third. This pressure has been exerted on certain mountains of Savoy. For example, the section which I have given [1] of the mountains situated between the Pointe-Percée and the neighbourhood of Bonneville enables it to be seen that those folded and contorted strata which are shown between Dessy and the Cal du Grand Barnaud cover a length which is two-thirds of that which they had before compression. These mountains, then, have been subjected, like the potter's clay, to a compression indicated by the ratio of 60 to 40. Contortions are not, perhaps, observed over all the surface of the globe; it has not been equally folded in all its extent, but they are found in a great number of countries, and even beneath strata almost horizontal. Sometimes the folds approach

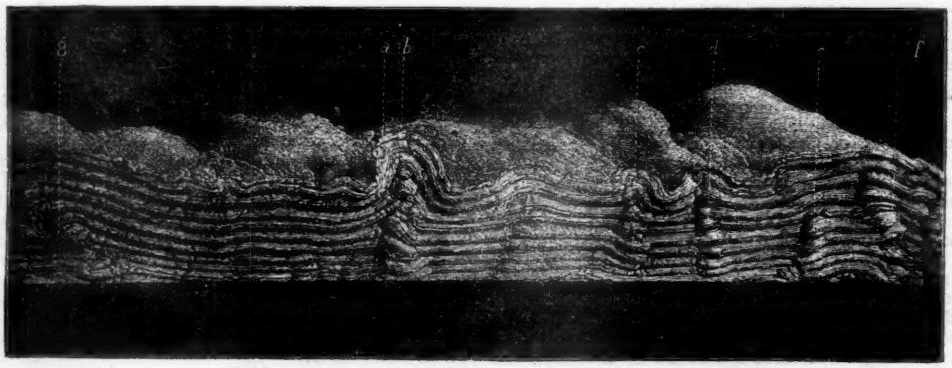

FIG. 2.

[1] *Bullet. Société Géologique de France*, 1875, t. iii. pl. xxii. A. Favre, *rches Géologiques*, Atlas, pl. ix.

the vertical, and are close against each other; this structure indicates that pressure has been exercised in a stronger manner than I have indicated.

"These powerful lateral thrusts of the external and solid parts of the globe appear to result from a diminution which the radius of the interior pasty or fluid nucleus has undergone during millions of ages. It may have been sufficiently great to cause the solid crust (which must always have been supported on the interior nucleus, whose volume continually diminishes) to assume the forms which we know, with a slowness equal to that of the contraction of the radius.

"To return to my experiments. At the extremities of the band of clay are pieces of wood or supports, which accompany it in its movement of contraction. The clay is thus compressed at once by its adhesion to the caoutchouc and by lateral pressure of the supports. By the influence of the caoutchouc alone, without the presence

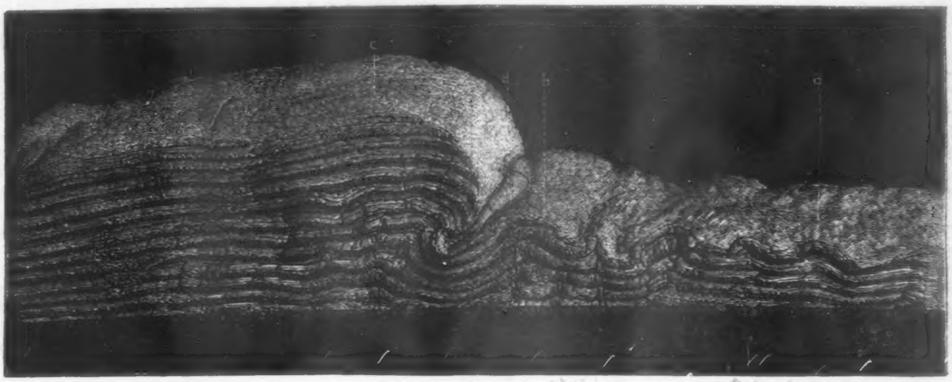

FIG. 3.

of the supports, there are formed only slight wrinkles on the surface of a sheet of clay 3 or 4 ctm. in thickness; and if the supports alone compressed the clay placed on a material which is not compressed (a very smooth oiled plate), the clay scarcely wrinkles near the centre of its surface; it increases a little in thickness and forms swellings (*bourrelets*) against the supports. The strata which appear to divide the masses of clay, and which are represented in the figures, are not really strata, but simply horizontal lines at the surface of the clay.

Such pressure as has been applied in these experiments produces contortions of strata which elevate the surface of the matter compressed, as well in the plane parts or plains, as in those which take the forms of valleys, hills, or mountains. These latter have the appearance of vaults or folds, sometimes perpendicular, sometimes warped (*déjétes*); the ridges are complete.

FIG. 4.

or broken at the summit by a longitudinal fracture, narrow below and wide above; next, another fracture, narrow above and wide below, is produced at the base of the mountain or vault. The sides of valleys are sometimes almost vertical, sometimes present gentle slopes. The strata are less strongly contorted in the lower parts than in the neighbourhood of the upper surface. They are disjoined in certain parts by fissures or caverns; they are traversed by clefts or faults inclined or vertical. All these deformations are the more varied in that they are not similar on the opposite sides of the same band of clay.

Most of these phenomena are seen in Fig. 1, which represents the result of an experiment made on a band of clay, whose thickness, before compression, was about 25 mm., while after that it attained 62 mm. at the culminating point. At *a* is seen a vault a little broken at the summit, covering a cavern similar to that figured in

the memoir of Sir J. Hall (*Trans.* R. S. E., vol. vii. 1813), and to that of the Petit Bornand in Savoy (Favre, *Recherches*, pl. x.); at *b* is a valley open at one of its ends and almost closed at the other; at *e* is a vault almost straight, the prolongation of which is very level; at *g*, *h*, and *l* are vaults twisted and a little broken, while at *i* is a broken fold, the curves of which are almost vertical. All these accidents of the ground recall those which have been so often observed in the Jura, the Alps, and the Appalachians.

Fig. 2 represents a band of clay whose thickness was about 40 mm. before compression, and 65 after. We remark contortions similar to those of the preceding figure, among others a vault *d*, very exactly formed. At distances are seen vertical slices, on which the pressure appears to have acted in a particularly energetic fashion, and which may be called "zones de refoulement;" the strata are there broken in an exceptional manner, often separated from each other. One of these vaults is replaced by a single fault on the opposite side of the band of clay.

Before compression, in the band of clay in Fig. 3, were seen the two divisions which are seen there now—that in the right was 33 cm. long, and 25 mm. thick at *a*, and 35 at *b*; the left division was 25 cm. long, and 65 mm. thick. A gentle slope united the part *c* to the part *b*. After compression, the mean height of *ab* was 45, and that of *c* 75 mm. All the layers were spread horizontally.

In this experiment I have sought to imitate the effect of crushing at the limit of a mountain and a plain. The height of the mountain *c* has been notably increased, the five or six upper layers have advanced on the side of the plain; they encroach on it. The plain has, however, offered a resistance sufficiently great to cause the strata of the mountain to be strongly inflected at the bottom. From this struggle between the plain and the mountain, there resulted a cushion, *d*, which is the first hill at the foot of the height. It also resulted that the strata of the plain assumed an appearance of depression at contact with the mountain in consequence of the vault which is formed at *b*; they plunge underneath the mountain. This resembles what is often seen in the Alps at the junction of the first calcareous chain and the hills of "mollasse;" in fact, the strata of the latter rock seem to plunge under those of the neighbouring heights. In consequence of the pressure, there are formed several ranges of hills in the plain between *b* and *a*.

"In Fig. 4 the band of clay had, before compression, a thickness of 45 mm.; after that the culminating point was more than 10 cm. I have here sought to represent what must happen when terrestrial pressure is exerted on horizontal strata still moist, deposited at the bottom of a sea where are two mountains already solidified. For this purpose I placed in the caoutchouc and under the clay two bare cylinders of wood, *a* and *b*, of about 35 mm. radius, at 20 cm. from the ends of the band of clay, and at the same distance from each other. Before compression, the surface of the clay and the strata were completely horizontal. Pressure gave rise at the top of the half-cylinder, *a*, to a valley, *c*, formed by a twisting of the beds to the right, and by a little mountain, *d*, to the left. But I do not believe that it has ever been thought to assign to a valley an origin of this nature.

"On the other semi-cylinder, *b*, is produced an enormous elevation which has carried the ground to *e*, with such a rupture that the left lip, *f*, *g*, has suffered a complete reversal by turning, as on a hinge, around the horizontal line which passes by the point *h*. It follows that the four upper strata of clay designated by the figures 1, 2, 3, 4, being in a normal position before compression, are, after that, so arranged as to show the succession represented by the following arrangement of figures:—1, 2, 3, 4, 4, 3, 2, 1, 1, 2, 3, 4, making the section of this formation by a line drawn from *x* to *z*. If the left lip should disappear we should then have between the points *x* and *z* the section 1, 2, 3, 4, 5, 1, 2, 3, 4, 5. Sections analogous to these, presenting inversions in the order of strata, are known to geologists.

"The forms assumed by the clay depend on several circumstances which it is difficult to describe, such as the strength and the rate of compression, the thickness and the greater or less plasticity of the clay, &c. Why have accidents of the upper surface of the clay, which are intimately connected with those of the interior of the mass, so small an extension that they are not even similar in the two sides of a band of clay? This small continuity is owing to causes which we can neither foresee nor appreciate. Is it not the same in nature? Why is the chain of the Alps not a true chain, but a succession of masses often oblique with respect to each other? Why, in the Jura, do we see chains which have for their prolongation plains and valleys? It is always the case that the forms and structures obtained in these experiments have an incredible resemblance to those which are found on the surface of the globe. But it must be admitted that many of the latter have not been reproduced by these artificial crushings.

"It appears probable that, by pressures more powerful and more variedly employed, we might obtain again very different structures. But I have not thought it necessary to multiply these experiments, thinking that the varied forms which have resulted show sufficiently the effects of crushing."

GEORGE HENRY LEWES

THIS is a name which has been long before the reading public of England, and the announcement of Mr. Lewes's death, on Saturday last, at the age of sixty-one years, will be received by very many with genuine regret. This will be especially the case with those who have reached or passed middle life, for latterly Mr. Lewes's name has come little before the public, and what work he has done appeals to a comparatively small circle. Of Mr. Lewes's many-sidedness every one knows; he commenced his career as a novelist, and ended as a physiological psychologist—perhaps in some respects no very great leap, after all; indeed the two functions may be said to be combined in that greatest of philosophical novelists, if not of novelists absolutely, "George Eliot," Mr. Lewes's widow. Science owes a good deal to Mr. Lewes; for, though he made little or no pretension to be an original investigator in physical science, he did very much by his writings to give the general public an idea of what real science is, and to help forward the good work of carrying it into every-day life. His "Physiology of Common Life" had a long and deserved popularity, and even yet, we believe, is often "asked for" at libraries and book-shops. His "Biographical History of Philosophy" is thoroughly readable and full of information, which is more than can be said of philosophical works generally. Of his "Life of Goethe," one of the very few masterly biographies, we leave it to others to speak, though he did much there to bring out the real importance of Goethe's botanical and other scientific researches. Of his latest work, "Problems of Life and Mind," we spoke at length on the appearance of the volumes that have been published; in these volumes and in one or two letters and articles contributed to our pages, Mr. Lewes was perhaps at his best as an investigator in a department of science with which we are cautious of interfering, but which has a strange fascination for many thinkers. Altogether Mr. Lewes filled an important and many-sided place in the intellectual life of this country during his long career. It is easy to say that a man of his unusual keenness of mind might have achieved permanent greatness by concentrating his great store of energy in one

particular direction; but then he would not have been the innately versatile George Henry Lewes. His influence has been spread over a wide field, and has been largely beneficial to progress and enlightenment; he never aimed, we believe, at piling up an enduring monument to himself.

NOTES

MR. E. J. STONE, F.R.S., Astronomer-Royal at the Cape of Good Hope, has been appointed to the Radcliffe Observatory, Oxford, in place of the late Rev. R. Main.

MR. WILLIAM SPOTTISWOODE, having been elected President of the Royal Society on Saturday last, has resigned the office of Secretary to the Royal Institution. At the meeting, on Monday last, it was proposed that the Members of the Royal Institution subscribe to present a bust of Mr. Spottiswoode to the Institution as a recognition of his valuable services as Treasurer and Secretary successively.

WE have already referred to the new Parkes Museum of Hygiene, at University College, London, and we now earnestly draw our readers' attention to the appeal made by the Executive Committee for subscriptions towards an endowment, which is absolutely necessary for the efficiency of the institution, in diffusing the much needed knowledge of sanitary appliances and their uses. Although quite in its infancy, the Parkes Museum contains objects relating to life-protection, dietetics, clothing, furnishing, engineering, and architecture—in fact, every branch of hygiene. The library already consists of between 300 and 400 volumes, exclusive of pamphlets. "It cannot be too widely known," the Executive Committee state, "that it is intended to extend the benefits of the Museum to all classes, so that not only professional men, but owners of property, employers of labour, artisans, and others, both men and women, may be able to study at their leisure the subjects in which they are most interested." The Executive Committee, therefore, confidently appeal for pecuniary support to all those who, while being interested in technical education and sanitary science, have the inclination and the means to give such assistance. The Committee will not only be glad to receive subscriptions of money, but also books and pamphlets in any language, statistical tables, maps, plans, and other drawings, models, apparatus, or specimens illustrating any branch of hygiene. Subscriptions may be paid to the Treasurer, Mr. Berkeley Hill, 55, Wimpole Street, W. All communications relating to the presentation of articles to the Museum should be addressed to the Curator, Mr. Mark H. Judge, Parkes Museum, University College, Gower Street, W.C.

THE *Daily News'* New York correspondent telegraphs that Mr. Edison announces that he has perfected a machine for measuring the current used in the electric light. It consists of an apparatus placed in every house lighted by electricity, which registers the quantity of electricity consumed, and uses for the purpose the one-thousandth part of the quantity consumed in the building. Mr. Edison declares that his invention of the light, including the arrangement for counteracting loss in subdivision, is now completed. His experiments at present are directed to reducing the cost. He has, he says, already brought this decidedly below the cost of gas, and as soon as the minimum is reached, will make the results public. The *New York World* contains the following interesting details of Mr. Edison's doings:—"Dozens of workmen and machinists are hard at work at Menlo Park on the new buildings, the workshop being now almost ready for the roof. Mr. Edison said to a reporter for the *World*, 'I don't know when I am going to stop making improvements on the electric light. I've just got another one that I found by accident. I was experimenting with one of my burners when I dropped a screw-driver on to it. Instantly the light was almost doubled and continued to burn with the increased power. I examined the burner and found it had been knocked out of shape. I restored it to its original form, and the light was decreased. Now, I make all my burners in the form accidentally given to that one by the screw-driver. The result is that I can produce the amount of light given out by the first burner with little more than half the power. It is almost impossible to calculate with certainty the resources of my light, but I have engaged a mathematician to work out the problem from my data.' On the whole, Mr. Edison states he is confident of success, however much any one may be puzzled by his methods or claims."

THE Corporation of Liverpool have given notice that they intend to apply in the ensuing Session for an Act authorising the lighting of the public streets, places, and buildings within the borough by means of the electric light, "or otherwise than by means of gas." The Corporations of Warrington, Derby, South Shields, Leicester, Blackburn, Over Darwen, and Stratford-upon-Avon, in conjunction, in the latter borough, with the Local Board of Health, ask for similar powers in the Bills which they intend to promote.

THE sixth part of the illustrated work of C. J. Maynard on the "Birds of Florida, and the Water and Game Birds of Eastern North America," has just been published, and contains three quarto plates, one of them representing sixty-six pecies of eggs.

MESSRS. MACMILLAN AND CO. will shortly publish "Notes of a Naturalist on Board the *Challenger*," by Mr. H. N. Moseley, F.R.S., who was on the scientific staff of the expedition. The work will be illustrated.

THE zoological station of the Zoological Society of the Netherlands has published its third report. During the summer of 1878 the station was erected on the Island of Terschelling, and in the course of two months it was visited by ten zoologists. This year the investigation of the Zuider-Zee was the principal object kept in view, and for that purpose some fourteen dredging excursions with the boat stationed in West-Terschelling for laying buoys were organised. The station underwent no small improvement, a nicely-organised aquarium-room being added to the main building. Here a small hot-air engine of about $\frac{1}{18}$-horse power (construction of D. W. van Rennes, Utrecht) drives an air-forcing pump; the compressed air gathered in a white-iron box is distributed through numerous aquarium-vessels by means of gum-elastic tubes and small glass-canules. By means of this arrangement even on hot days numerous animals were kept alive. The investigation of the Zuider-Zee not being brought to a close, the Island of Terschelling will probably next year see the station again erected in one of its picturesque valleys.

AT last week's meeting of the Paris Academy M. Pasteur read a critical examination of the posthumous papers of Claude Bernard, in which statements were made opposed to the conclusions reached by M. Pasteur. He regards the manuscript of Bernard as a sterile attempt to substitute for well-established facts the deductions of an ephemeral system. "The errors, however," M. Pasteur says, "of those who in the sciences have accomplished a valiant career have only the philosophical interest which attaches to the knowledge of our human frailty."

THE following are the probable arrangements of the Royal Institution for the Friday Evening Meetings before Easter, 1879:—January 17, Prof. Tyndall, "The Electric Light;" January 24, Prof. W. E. Ayrton, "The Mirror of Japan and its Magic Quality;" January 31, Mr. H. Heathcote Statham,

"The Logic of Architectural Design;" February 7, Rev. H. R. Haweis, "Bells;" February 14, Prof. G. Johnstone Stoney, "The Story of the November Meteors;" February 21, Prof. Roscoe, "A New Chemical Industry;" February 28, Sir Wm. Thomson, "The Sorting Demon of Maxwell;" March 7, Prof. Huxley, LL.D., F.R.S.; March 14, Mr. E. B. Tylor, "The History of Games;" March 21, Prof. Abel, C.B., F.R.S., "Recent Contributions to the History of Detonating Agents;" March 28, Sir Henry C. Rawlinson, K.C.B., D.C.L., "The Geography of the Oxus, and the Changes of its Course at different Periods of History;" April 4, Mr. Warren de la Rue, D.C.L., F.R.S.

THE Smithsonian Institution of Washington, U.S., has issued a catalogue of their valuable publications, and a list of various societies, journals, &c., with which they exchange publications. The Institution is anxious to add to this list any societies with which they have not hitherto been in communication, and also the names of specialists in all parts of the world. Communications should be addressed to Dr. Spencer F. Baird, at the Institution.

THE Rev. S. J. Whitmee writes us with reference to reports which have lately reached this country of violent volcanic eruptions in the Society Islands, and also in the neighbourhood of New Britain. "These," he says, "have not, however, been confirmed, and from private information received from Tahiti, I hear that nothing has occurred in the Society Islands like what has been reported. From an Australian paper I saw a month ago, that the master of the brigantine *Matautu* had found a vast quantity of pumice-stone about the Ellice Islands. This is now confirmed by the captain of the missionary barque *John Williams*, who visited these islands in May and June last. From his account, a brief notice of which appeared in the *Times* last Friday, we learn that the pumice has reached the Ellice group from another locality, and that there has been no eruption in those islands. I give the following quotation from a notice of the *John Williams*' voyage which appears in the *Samoa Times* for July 27:—'In the Gilbert group, although the currents were somewhat irregular, the old strong easterly set seems to prevail again. In fact, sometimes it was running at the rate of thirty-six to forty-eight knots in the twenty-four hours. . . . On the outward voyage (from Samoa), when about 120 miles to the east of Nukulaelae, vast quantities of pumice-stone were passed, and the shores of all the Ellice Islands from Nukulaelae to Nintao and Nanumea are thickly covered with it. It is no exaggeration to say that hundreds of tons have been thrown up on every island. Stumps of trees and thick bamboos with roots attached have been thrown up on some of the islands. Early in May the brig *Isabella*, Everts, master, from Sydney, called at Vaitupu, and there left a report that there had been some volcanic eruption in the Society group. One report even says that Raiatea and Borabora have been destroyed, and 2,000 lives lost. There were also passed while beating back (to Samoa) between Nukulaelae and Fakaofo, one *mălili* tree about eighty feet long, one cocoa-nut tree, and four other gigantic forest trees, all evidently proofs that the story of Capt. Evers is probably only too true. The pumice-stone began to arrive on the Ellice Islands about the middle of April, and continued till the middle of June. When first noticed there was no seaweed growth on the pieces, and no barnacles, but by the beginning of June this began to be plainly noticeable. There is no trace of the pumice-stone in the Union or Gilbert Groups, and the time of its arrival in the Ellice group will be interesting to those who study the question of ocean currents.' From the above remarks it would appear that the pumice is supposed to have arrived from the east, and it is taken for granted that it has come from the Society Islands. But it is at least doubtful whether any eruption has taken place there. And, at a date later than that given as the time when the pumice ceased to reach Vaitupu, the *John Williams* met with trees far to the east of that island. If it had come from the east, surely some would have been seen also about the Union or Tokelau Islands. We must wait for further information as to where the eruption took place; but I am inclined to think it must have been to the west of the Ellice Islands."

WE hear that M. Mannheim is engaged upon the preparation of a work entitled "Géométrie Cinématique." Hitherto our acquaintance with this important subject has been derived from Italian and German works, or these translated into French. M. Mannheim's ability as a geometer leads us to expect he will produce an elegant treatise on this branch of geometry.

ACCORDING to the report for 1877 recently issued, made by Dr. Corfield, the Medical Officer of Health for St. George's, the death-rate for the parish in that year was as low as 17·46. Of the three sub-districts into which the parish is divided Hanover Square had a rate of 16·58, Mayfair of 13·04, and Belgravia of 18·70. The rate for the whole parish (17·46), with a calculated population of 91,037 is compared with some of the twenty-two "other large towns of the United Kingdom." Portsmouth has the lowest rate (17·4), which is therefore just under, Brighton with 18·7 coming next. No foreign town has so low a death rate as St. George's parish, the nearest known approach being Philadelphia (18·8). Dr. Corfield has calculated the mean duration of life in the parish and in each of the sub-districts for 1877. For the whole parish it was 49·52 years, for Hanover Square sub-district 56·63, for Mayfair 62·66, and for Belgravia 45·53. The mean duration in the 22 towns on which the Registrar-General reports was (including London) 34·2.

PROF. E. MORREN'S useful "Correspondance Botanique" is continually increasing in size. This year it occupies 150 pages, against 92 last year. The principal increase is in the American department, which is much fuller and more complete than it has been before. But in Europe also there is an increase in the number of names, partly due no doubt to more accurate information, but partly also, we may hope, to an actual increase in the number of workers in botanical science. It is a significant fact that while second and third class university towns in Germany or Italy number their six or eight working botanists, mostly attached to the university, the number of names given under the heads of Oxford and Cambridge together is five.

THE *Golos* publishes the following telegram from Tiflis, dated November 27:—"Telegraphic intelligence received to-day states that this morning at two o'clock a severe earthquake was experienced at Suram and Borjom. A frightful rumbling noise was heard during the earthquake, accompanied by a shock so severe that people were thrown from their beds. Nothing similar has occurred in the district before."

YESTERDAY M. Bardoux, Minister of Public Instruction, laid the first stone of the new practical school of the Faculty of Medicine in Paris.

THE Commission for the reception of the great reflector at the Paris Observatory have assented to its reception, although the mirror cannot be used in its full breadth without being diaphragmised at the circumference. It is stated, moreover, that the instrument can be used only on a very few days in the year in the atmosphere of Paris, and that reflectors are decidedly unmanageable except under special circumstances. It remains to be explained, then, how Lord Rosse and the great Herschel could manage to make so many interesting discoveries with instruments declared so unsuitable for celestial observations.

THE Paris Observatory being no more intrusted with the care of publishing the *International Bulletin of Meteorology*, which has been taken over to the rue de Grenelle by the Central

Bureau, the Paris Press are deprived of all the documents for current meteorology which were put at their disposition by M. Leverrier.

WE are informed that Admiral Mouchez has signed with M. Martin the contract for the polishing of the great lens of the great refracting telescope prepared by M. Leverrier. The lamp, of 75 centimetres diameter, has been placed in the hands of M. Feil, the glass founder, to repair a few defects which have been detected. This operation is done by cutting out the defective parts and heating the glass to a state of liquidity. This process is sometimes used for central parts with success. Guinault, the originator of the process used now for glass-founding, is said to have so mended eighteen times one of the most celebrated glasses produced by him at the end of the last century.

WE regret to announce the death of Dr. Eugen von Gorup Besanez, Professor of Chemistry at Erlangen University, and author of an excellent chemical handbook in three volumes. He had attained the age of sixty-two years.

THE Italian traveller, Odoardo Beccari, writing from Sumatra, reports the discovery of a species of the genus *Amorphophallus* (of the Aroideæ family), the blossom of which ssurpasses in size that of *Victoria regia*. It appears that this magnificent plant, which Beccari baptises "Titanum," grows in the forests of Sumatra, and that its funnel-shaped calyx has a depth of 1·72 metres, while the diameter of the flower is 83 centimetres.

NEAR the Rhinefall at Schaffhausen a cave has been discovered which was evidently used as a dwelling-place in prehistoric times. Flints, broken jars, and bone rests were found in it. The jars were partly of Celtic and Roman origin.

WE have on our table the following works:—"Six Months in Ascension," by Mrs. Gill, John Murray; "Robert Dick, Geologist and Botanist," by Dr. Smiles, John Murray; "Pocket Book for Chemists," by Thomas Bayley, E. and F. H. Spon; "The Mollusca of the Firth of Clyde," by Alf. Brown, Hugh Hopkins; "A Visit to South America," by Edwin Clark, Dean and Son; "Coal: its History and Uses," Edited by Prof. Thorpe, F.R.S., Macmillan and Co.; "Bau des Eozoon Canadense," by Karl Möbius, Theodore Fischer; "Flowers, and their Unbidden Guests," Translated by W. Ogle, Kegan Paul; "Vogelbilder aus fernen Zonen," by Dr. Ant. Reichenon, Theodore Fischer; "Dictionary of Scientific Terms," by William Rossiter, W. Collins and Sons; "On Foot in Spain," by J. S. Campion, Chapman and Hall; "Elementary Geometry Books," i.-v., Fourth Edition, by J. M. Wilson, Macmillan and Co.; "Treatise on Chemistry," vol. ii., part 1, by Professors Roscoe and Schorlemmer, Macmillan and Co.; "The Magic Lantern Manual," by W. J. Chadwick, F. Warne and Co.; "The Localisation of Cerebral Disease," by Dr. Ferrier, Smith, Elder, and Co.; "Cassell's Natural History," vol. ii., Edited by P. Martin Duncan, F.R.S., Cassell, Petter, and Galpin.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Dr. Whately, R.N.; a Brazilian Tree Porcupine (*Sphingurus prehensilis*) from Trinidad, presented by Dr. J. F. Chittenden, jun.; a Common Peafowl (*Pavo cristatus*) from India, presented by Mrs. Russ; two Common Cormorants (*Phalacrocorax carbo*), British, presented by Mr. Frank Buckland, F.Z.S.; a Water Rail (*Rallus aquaticus*), British, presented by Mr. W. Thompson; a Black Lemur (*Lemur macaco*) from Madagascar, a Rufous-vented Guan (*Penelope cristata*) from Central America, purchased; a Red Kangaroo (*Macropus rufus*), born in the Gardens.

ROYAL SOCIETY—THE PRESIDENT'S ANNIVERSARY ADDRESS[1]

GENTLEMEN,

AT the conclusion of this, the fifth and last year during which I shall have held the most honourable office of your president, I have the gratifying assurance that the communications made to the Society and its publications have in no respect fallen off in scientific interest and value. We have not, indeed, been called upon to undertake during the past year such responsible and time-absorbing duties in behalf of the Government as the Polar, Circumnavigation, Transit of Venus, and other Committees demanded of us during the previous four years; but some of the results already achieved by those expeditions have been contributed to our publications, and we are in expectation of more. It is also with satisfaction that I can refer to the good attendance at our evening meetings, *soirées*, and *réunions*, as evidence of the interest taken in our proceedings by the Fellows generally and their friends.

Before proceeding to touch upon some of the advances made in science during the last few years, I have, as heretofore, to inform you of the Society's condition and prospects, and of those duties undertaken by its Council, for information as to which non-resident Fellows look to the annual address.

The loss by death of Fellows, twenty-one in number, is but little short of last year's rate, while that of Foreign Fellows (six) is twice as great as last year. On the home list is Sir George Back, the last, with the exception of our late venerable President, Sir E. Sabine, of that celebrated band of Arctic voyagers which during the early part of the century added so much to our renown as navigators and discoverers. Sir George was further the companion of Franklin and Richardson in that overland journey to the American Polar Sea, in which human endurance was tried to the uttermost compatible with human existence, and the modest but thrilling narrative of which, by the first-named officers, will ever hold a unique place in the annals of scientific discovery. Of Indian explorers no less than four have been taken away, namely, Col. Sir Andrew Waugh, for many years Director of the Great Trigonometrical Survey of India; and shortly afterwards his successor, Col. Montgomerie; Dr. Oldham, for a quarter of a century the Director of the Geological Society of India; and Dr. Thomas Thomson, my fellow-traveller in the Himalaya, the narrative of whose explorations in Western Tibet contains the first connected account of the physical and natural features of that remote and difficult country. Lieut.-Gen. Cameron survived but for one year our late Fellow, Sir Henry James, his predecessor in the Direction of the Ordnance Survey of Great Britain. In the Rev. James Booth we have lost a mathematician of high attainments, and the author of many contributions to our own and other scientific journals. The Rev. W. B. Clark, of New South Wales, wrote many papers abounding in excellent observations on Meteorology and Geology, especially made in England, the Cape of Good Hope, Australia, and the Pacific. The Rev. R. Main, the Director of the Radcliffe Observatory, at Oxford, was a very eminent astronomer, and for nearly half a century an indefatigable author. Lastly, Earl Russell, the distinguished statesman and the earnest advocate, whether in the Government or in Parliament, of every measure for the promotion of scientific inquiry.

Of Foreign Fellows our losses are a great chemist in Becquerel, of Paris, whose election took place upwards of forty years ago; a great physiologist in Claude Bernard, also of Paris; the father of mycology, and for long the patriarch of Scandinavian botanists, Elias Fries; a most distinguished physicist and the recipient of both a Rumford and Copley medal in Regnault; a veteran anatomist in Weber; and in Secchi, of Rome, an astronomer of astonishing activity, the author of more than three hundred separate contributions to the science of which he was so great an ornament.

In matters of finance I may with satisfaction refer you to our treasurer's balance-sheets.

It will be in your recollection that Mr. T. J. Phillips Jodrell placed in 1874 a sum of 6,000*l.* at the disposal of the Society, with the view of its being devoted to the encouragement of scientific research by periodical grants to investigators whom your Council might think it expedient thus to aid. Shortly after the receipt of this munificent gift, the Government announced its

[1] Address of Sir Joseph Hooker, C.B., K.C.S.I., the President, delivered at the Anniversary Meeting of the Royal Society, on Saturday, November 30, 1878.

intention of devoting annually for five years 4,000*l*. to the same object, thus anticipating the special purpose which Mr. Jodrell had in view. Thereupon, with that gentleman's consent, his donation was temporarily funded, and the proceeds applied to the general purposes of the Society until some other scheme for its appropriation shall be approved. In April last I received a further communication from Mr. Jodrell, declaring it to be his wish and intention that, subject to any appropriation of the sum which we might, with the approval of the Society, make during his lifetime, it should immediately on his death be incorporated with the Donation Fund, the annual income in the meantime going to the general revenue of the Society. Upon this subject I have now to state that since the receipt of that letter Mr. Jodrell has approved of 1,000*l*. of the sum being contributed to a fund presently to be mentioned.

I have also to inform you of a check for 1,000*l*. having been placed in my hands by our Fellow, Mr. James Young, of Kelly, to be expended in the interests of the Society in such manner as I should approve.

Mr. De La Rue, to whose beautiful experiments I shall have occasion to refer, has presented to the Society both the letterpress and the exquisitely engraved facsimiles of the electric discharges described in his and Dr. Hugo Müller's paper, and which have appeared with that paper in the *Transactions*.

Our Fellow, Dr. Bigsby, has presented seven copies of his "Thesaurus Devonico-carboniferus" for distribution, and they have been distributed accordingly.

A very valuable addition to our gallery of deceased eminent Fellows has been the gift, by Mr. Leonard Lyell, of a copy in marble, by Theed, of the bust of his uncle, the late Sir Charles Lyell, F.R.S., together with a pedestal. This is the best likeness of the late eminent geologist that has been executed, and is in every respect a satisfactory one.

I have the gratification of announcing to you, that through the munificence of a limited number of Fellows, means have been advanced for reducing the fees to which all ordinary Fellows in future elected will be liable. That these fees, though not higher than the most economical expenditure on the part of the Society for its special purposes demanded, were higher than it was expedient to maintain if any possible means for reducing them could be obtained, was not only my own opinion but that of many Fellows. They exceed considerably those of any other scientific society in England or abroad; their amount has occasionally prevented men of great merit from having their names brought forward as candidates, and they press heavily, especially upon those who, with limited incomes, have other scientific societies to subscribe to. Nor does it appear to me as otherwise than regrettable that so high an honour as Fellowship of the Royal Society, the only one of the kind in England that is limited as to the number annually elected, and selective in principle, should be attainable only at a heavy pecuniary expenditure. It is true that our Fellows receive in return annually volumes of publications of great value to science generally; but these treat of so many branches of knowledge that it is but a fraction of each that can materially benefit the recipient, while their bulk entails an additional expenditure; and now that the individual papers published in the *Transactions* are separately obtainable, the advantages of Fellowship are less than they were when to obtain a treatise on his own subject a specialist had either to join the Society, or to purchase a whole volume or a large part of it annually.

It was not, however, till I had satisfied myself that the annual income of the Society, though not ample, was sufficient for its ordinary purposes, that its prospects in other points of view were good, and that the expenditure upon publication was the main, if not the sole, obstacle to a reduction of fees, that I consulted your treasurer on the subject of taking steps to attain this object.

My first idea was to create, by contributions of small amount, a fund, the interest of which should be allowed to accumulate; and when the income of the accumulated capital reached a sufficient amount to enable the Society to take the step without loss of income, to reduce either the entrance fee or annual contribution; and to which fund Mr. Young's gift should be regarded as the first donation.

This proposal was in so far entertained by your Council that they resolved to establish a Publication Fund, and to place Mr. Young's gift to the credit thereof; and further, appointed a committee to consider and report upon the Statutes of the Society concerning the fees.

The movement, once set on foot, met with an unexpectedly enthusiastic reception; several Fellows, with the best means of forming a judgment, not only approved of it, but offered liberal aid, urging that the reduction of fees should be the first and immediate object, and that, if such a course were thought desirable, the means of carrying it out would surely be forthcoming. On this your Treasurer prepared for my consideration a plan for raising 10,000*l*., the sum required for effecting any material reduction; and we resolved to ascertain by private inquiry whether so large an amount could be obtained.

Here again our inquiries were responded to in a spirit of, I may say, unexampled liberality: in a few weeks upwards of 8,000*l*. was given or promised by twenty Fellows of the Society, and I need hardly add that the remaining 2,000*l*. was contributed very shortly afterwards.

At a subsequent meeting of the Council it was resolved:—

1.—That the sums referred to as the Publication Fund, as well as those received or that may be hereafter received, for the purpose of relieving future ordinary Fellows from the Entrance Fee, and for reducing their Annual Contribution, be formed into one fund.

2.—That the Entrance Fee for ordinary Fellows be henceforth abolished; and that the Annual Contribution for ordinary Fellows hereafter elected be three pounds (3*l*.). Also, that the income of the Fund above-mentioned be applied, so far as is requisite, to make up the loss to the Society arising from these remissions and reductions.

3.—That the account of this Fund be kept separate; and that the annual surplus of income, after providing for the remission and reduction above recommended, be re-invested, until the income from the Fund reaches 600*l*. So soon as the annual income reaches this amount, any surplus of income in any year, after providing for the remission and reduction above-mentioned, shall be available, in the first instance, in aid of publication and for the promotion of research.

A list of subscribers to this Fund will be placed in the hands of every Fellow, with the information that it will be kept open for future contributions, in the interests of research and of the Society's publications. I hope that it will be largely and speedily augmented, and that it may eventually reach an amount which will provide us with the means of accomplishing as much as is effected by the Government Fund, upon our own sole and undivided responsibility. I must not conclude my notice of this movement without a mention of those whose encouragement and liberality have most largely promoted it; and first of all, Mr. Spottiswoode, to whose counsel and active co-operation throughout, its success is mainly due; Messrs. Young's and Jodrell's contributions have already been alluded to, they have been supported by others:—2,000*l*. from Sir Joseph Whitworth, 1,000*l*. from Sir W. Armstrong, and 500*l*. each from His Grace the Duke of Devonshire, Mr. De La Rue, Messrs. Spottiswoode and Eyre, Dr. Siemens, and the Earl of Derby, and 250*l*. from Dr. Gladstone. The balance is the joint contributions of thirty-two Fellows.

I have to mention your obligations to Dr. W. Farr for the labour he has bestowed on ascertaining these vital and other statistics of the Society, upon an accurate knowledge of which the calculations for the reduction of fees had to be based; and to Mr. Bramwell for constructing a table showing to what extent the above changes will affect the Society's present and future income. It may interest you to know that the contributions of ordinary Fellows in future to be elected, is but little over that which was required of all Fellows from the very commencement of the Society's existence, namely, 1*s*. per week, and that the last Fellow who paid that sum died in 1869. So recent (1823) has been the augmented scale of payment in force up to the present date.

Looking back over the five years during which I have occupied this chair I recognise advances in scientific discovery and research at home and abroad far greater than any previous semi-decade can show. I do not here allude to such inventions as the telephone, phonograph, and microphone, wonderful as they are, and promising immediate results of great importance to the community, nor even to those outcomes of great research and high attainments, the harmonic analyser of Sir W. Thomson, the radiometer and otheoscope of Crookes, the bathometer and gravitation meter of Siemens, but to those discoveries and advances which appeal to the seeker of knowledge for its own sake, whether as developing principles, suggesting new fields of

research, or awakening attention to hitherto unseen or unrecognised, or unexplained phenomena of nature.

In the foremost rank as regards the magnitude of the undertakings and the combination of means to carry them out, nothing in the history of physical science can compare with the Transit of Venus Expeditions. To observe the transit of Venus various nations of Europe and the United States competed as to the completeness of the expeditions they severally equipped. The value[1] of the solar parallax cannot be ascertained until the results of all the expeditions are taken into account, when it will have an international claim to acceptance. But advances in this direction will not have ended here, the very difficulties attending the observation of the transit of Venus, having directed attention to the method originally suggested by the Astronomer-Royal in 1857, of obtaining the solar parallax from the diurnal parallax of Mars at its opposition.

Mr. Gill, by the skilful employment at Ascension Island of the heliometer lent by Lord Lindsay, has greatly increased the accuracy of the method by which the necessary star comparisons with Mars are made, and there is every reason to believe that the results of his observations, which are now in course of reduction, will be very satisfactory.

Within the last two years a remarkable addition has been made to the number of members of the solar system by Prof. Asaph Hall's discovery of the satellites of Mars; and more recently, during the solar eclipse which was visible in America, by Prof. Watson's detection of planetary bodies within the orbit of mercury.

In 1876 Schmidt recorded an outburst of light in a star in Cygnus, which showed a continuous spectrum containing bright lines similar to those of the remarkable star of 1866. As the star waned the continuous spectrum and bright lines faded, all but one bright line in the green, giving the object the spectroscopic appearance of a small gaseous nebula.

Great progress has been made during the last five years at Greenwich in the method of determining the motions of the heavenly bodies by the displacement of the lines in their spectra, as first successfully accomplished by Mr. Huggins in 1868. Not only do the results obtained by the stars observed at Greenwich agree with those of Mr. Huggins, as satisfactorily as can be expected in so delicate an investigation, but the motions of seventeen more have been determined; whilst the trustworthiness of the method has been shown by the agreement of the values for the rotation of the sun and the motions of Venus, with the known movements of these bodies. Mr. Huggins has also obtained photographs of the spectra of some of the brighter stars, which give well-defined lines in the violet and ultra-violet parts of the spectrum. These spectra have already shown alterations in the lines common to them and the sun, which are of much interest.

In solar physics, which afford remarkable evidence of Mr. Lockyer's energetic labours in this country and Mr. Janssen's in France, I must mention our Foreign Member's wonderful photographs of the sun, wherein the minutest of the constant changes in the granulations exhibited on its surface (and which vary in size from $\frac{1}{10}$ of a second to 3 or 4 seconds) can be studied in the future from hour to hour and day to day; as can also their different behaviour at different periods of frequency of sunspots.

Before dismissing this fruitful field of research, I must allude to Mr. Lockyer's discovery of carbon in the sun; and to his announced but not yet published observations on the changes and modifications of spectra under different conditions, some of which he even regards as indicating the breaking up of the atoms of bodies hitherto regarded as elementary.

Some important investigations on the electric discharge have been communicated to the Society by Messrs. De La Rue and Müller, and by Mr. Spottiswoode. These, prosecuted by different means, tend to limit the possible causes of the stratification observed in discharges through vacuum tubes. They also point to the conclusion that this phenomenon is in a great measure due to motions among the molecules of the residual gas, which themselves become vehicles for the transmission of electricity through the tube. It is well known that gases at atmospheric pressure offer great resistance to the passage of electricity; and that this resistance diminishes (to a certain limit, different for different gases) with the pressure. And the researches in question appear to show that the discharge, manifestly disruptive at the higher pressures, is really also disruptive even at pressures when stratification takes place. The period of these discontinuous discharges has not yet been the subject of measurement, but it must, in any case, be of a very high order.

Under the auspices of the Elder Brethren of the Trinity House, and as their scientific adviser, Prof. Tyndall has conducted an investigation on the acoustic properties of the atmosphere. The instruments employed included steam whistles, trumpets, steam syrens, and guns. The propagation of sound through fog was proved to depend not upon the suspended aqueous particles, but upon the condition of the sustaining air. And as air of great homogeneity is the usual associate of fog, such a medium is often astonishingly transparent to sound. Hail, rain, snow, and ordinary misty weather, were also proved to offer no sensible obstruction to the passage of sound. Every phenomenon observed upon the large scale was afterwards reproduced experimentally. Clouds, fumes, and artificial showers of rain, hail, and snow were proved quite ineffectual to stop the sound, so long as the air was homogeneous, while the introduction of a couple of burners into a space filled with acoustically transparent air soon rendered it impervious to the waves of sound. As long as the continuity of the air in their interstices was preserved, the sound-waves passed freely through silk, flannel, green baize, even through masses of hard felt half an inch in thickness, the same sound-waves being intercepted by goldbeater's skin. A cambric handkerchief which, when dry, offered no impediment to their passage, when dipped into water became an impassable barrier to the sound-waves.

Echoes of extraordinary intensity were sent back from non-homogeneous transparent air; while similar echoes were afterwards obtained from the air of the laboratory, rendered non-homogeneous by artificial means. Detached masses of non-homogeneous air often drift through the atmosphere, as clouds pass over the face of the sky. This has been proved by the fluctuations observed with bells having their clappers adjusted mechanically, so as to give a uniform stroke. The fluctuations occur only on certain days; they occur when care has been taken to perfectly damp the bell between every two succeeding strokes; and they also occur when the direction of the sound is at right angles to that of the wind. Numerous observations were also made on the influence of the wind, the results obtained by previous observers being thereby confirmed. From his own observations, as well as from the antecedent ones of Mr. Alexander Beazeley and Prof. Osborne Reynolds, Prof. Tyndall concludes that the explanation of this phenomenon given by Prof. Stokes is the true one.

Turning now to biological branches of science, I find that the discoveries and researches of the past five years in this department also are far in advance of those of any previous period of equal length. The *Challenger* Expedition was, in point of the magnitude of the undertaking and completeness of its equipment, the rival of that for observing the Transit of Venus. Its general results, as far as hitherto made known, have been dwelt upon at length in my previous addresses, and the publication of them in detail is being rapidly pushed forward. Some very important papers by Mr. Moseley on the corals collected on the voyage have indeed been published in our *Transactions* with admirable illustrations by himself.

To the botanist and geologist no subject has a greater interest than that of the conditions under which the successive floras, which inhabited the polar area, existed and were successively dispersed over lower latitudes previous to their extinction, some *in toto* and over the whole globe, whilst others, though extinct in the regions where they once flourished, exist now only in lower latitudes under identical or under representative forms. It is only during the last few years that, thanks to the labours of those engaged in systematic botany in tracing accurately the directions of migrations of existing genera and species, and in determining the affinities of the extinct ones, and of palæontologists in referring the latter to their respective geological horizons, that any material advance has been made towards a knowledge of the origin and distribution of earlier and later Floras. I cannot better illustrate the condition of this inquiry than by calling your attention to two most recent publications on the subject, which have both appeared within the last few months.

[1] The Astronomer-Royal informs me that Capt. Tupman, who has taken the principal share in the superintendence of the calculation, fixes provisionally on a mean parallax of 8″·8455, corresponding to a distance of 92,400,000 British miles, but that the observations would be fairly satisfied by any parallax between 8″·82 and 8″·88, which in distance produces a range of from 92,044,000 and 92,770,000 miles, differing by 726,000 miles, quantity almost equal to the sun's diameter.

As a contribution to the principles of geographical botany, Comte Gaston de Saporta's essay, entitled "L'Ancienne Végétation Polaire" (which appeared in the *Comptes Rendus* of the French International Geographical Congress) is a very suggestive one, and, having regard especially to its author's eminence as a geologist and palæontologist, is sure to command attentive study. Although it may be argued that neither solar nor terrestrial physics, nor geology, nor palæontology are in a sufficiently advanced condition to warrant the acceptation as altogether established truths of all conclusions advanced in it, still the array of facts adduced in evidence of these conclusions is very imposing, while the ability and adroitness with which they are brought to bear on the subject are almost worthy of the great French genius whose speculations from the starting-point of the theory, which is that life appeared first in the northern circumpolar area of the globe, and that this was the birthplace of the first and of all subsequent floras.

I should premise that Count Saporta professedly bases his speculations upon the labours of his friend, Professor Heer, whose reasonings and speculations he ever puts forward with generous appreciation, while differing from him wholly on the subject of evolution, of which he is an uncomprising supporter, Professor Heer holding to the doctrine of the sporadic creation of species.

In his "Epoques de la Nature" Buffon argues that the cooling of the globe, having been a gradual process, the polar regions must have been the first in which the heat was sufficiently moderate for life to have appeared upon it; that other regions being as yet too hot to give origin to organised beings, a long period must have elapsed, during which the northern regions, being no longer incandescent, as they and all others originally were, must have had the same temperature as the most tropical regions now possess.

Starting from this thesis, Count Saporta proceeds to assume that the termination of the azoic period coincided with a cooling of the water to the point at which the coagulation of albumen does not take place; and that then organic life appeared, not in contact with the atmosphere, but in the water itself. Not only does he regard life as originating, if not at the North Pole, at least near to it, but he holds that for a long period life was active and reproductive only there. In evidence of this he cites various geological facts, as that the older, and at the same time the richest, fossiliferous beds are found in the cool latitudes of the North, namely in lats. 50° to 60°, and beyond them. It is in the North, he says, that Silurian formations occur, and though they extend as far south as lat. 35° N. in Spain and America, the most characteristic beds are found in Bohemia, England, Scandinavia, and the United States. The Laurentian rocks again, he says, reach their highest development in Canada, and palæozoic rocks cover a considerable polar area north of the American great lakes, and appear in the coasts of Baffin's Bay, and in parts of Greenland and Spitzbergen. It is the same with the Upper Devonian and marine carboniferous beds preceding the coal formations; these extend to 76° N. in the polar islands and in Greenland, and to 79° N. in Spitzbergen, and he adds that M. d'Archiac has long ago remarked that, though so continuous to the southward of the northward, the coal-beds become exceptional to the southward of 35° N. Hence Count Saporta concludes that the climatic conditions favourable to the formation of coal were not everywhere prevalent on the globe, for that while the southern limit of this formation may be approximately drawn, its northern must have extended to the Pole itself.

I pass over Saporta's speculations regarding the initial conditions of terrestrial life, which followed upon the emergence of the earlier stratified rocks from the Polar Ocean, and proceed to his discussion of the climate of the carboniferous epoch as indicated by the characters of its vegetation, and of the conditions under which alone he conceives this can have flourished in latitudes now continuously deprived of solar light throughout many months of the year. In the first place, he accepts Heer's conclusions (founded on the presence of a tree-fern in the coal measures specifically similar to an existing tropical one), that the climate was warm, moist, and equable, and continuously so over the whole globe, without distinction of latitude. This leads him to ask whether, when the polar regions were inhabited by the same species as Europe itself, they could have been exposed to conditions which turned their summers into a day of many months' duration, and their winters into a night of proportional length?

A temperature so equable throughout the year as to favour a rich growth of cryptogamic plants, appears, he says, to be at first sight incompatible with such alternating conditions (as a winter of one long night and a summer of one long day); but equability, even in high latitudes, may be produced by the effect of fogs due to southerly warm oceanic currents, such as bathe the Orkneys and even Bear Island (in lat. 75° N.), and render their summers cool and winters mild. To the direct effects of these he would add the action of such fogs in preventing terrestrial radiation, and hence the cold this produces; and he would further efface the existing conditions of a long winter darkness by the hypothesis that the solar light was not, during the formation of the coal, distributed over the globe as it now is, but was far more diffusive, the solar body not having yet arrived at its present state of condensation.

That the polar area was the centre of origination for the successive phases of vegetation that have appeared in the globe is evidenced, under Count Saporta's view, by the fact that all formations, carboniferous, jurassic, cretaceous, and tertiary, are alike abundantly represented in the rocks of that area, and that, in each case, their constituents closely resemble that of much lower latitudes. The first indications of the climate cooling in these regions is afforded by *Coniferæ*, which appear in the polar lower cretaceous formations. These are followed by the first appearance of dicotyledons with deciduous leaves, which again marks the period when the summer and winter season first became strongly contrasted. The introduction of these (deciduous-leaved trees) he regards as the greatest revolution in vegetation that the world has seen; and he conceives that once evolved they increased, both in multiplicity and diversity of form, with great rapidity, and not in one spot only, and continued to do so down to the present time.

Lastly, the advent of the miocene period, in the polar area, was accompanied with the production of a profusion of genera, the majority of which have existing representatives which must now be sought in a latitude 40° further south, and to which they were driven by the advent and advance of the glacial cold; and here Count Saporta's conclusions accord with those of Prof. A. Gray, who first showed, now twenty years ago, that the representatives of the elements of the United States flora previously inhabited high northern latitudes, from which they were driven south during the glacial period.

Perhaps the most novel idea in Count Saporta's essay is that of the diffused sunlight which (with a densely clouded atmosphere), the author assumes to have been operative in reducing the contrast between the polar summers and winters. If it be accepted it at once disposes of the difficulty of admitting that evergreen trees survived a long polar winter of total darkness, and summer of constant stimulation by bright sunlight; and if, further, it is admitted that it is to internal heat we may ascribe the tropical aspect of the former vegetation of the polar region, then there is no necessity for assuming that the solar system at those periods was in a warmer area of stellar space, or that the position of the poles was altered, to account for the high temperature of pre-glacial times in high northern latitudes; or, lastly, that the main features of the great continents and oceans were very different in early geological times from what they now are. Count Saporta's views in certain points coincide with those of Prof. Le Conte of California, who holds that the uniformity of climates during earlier conditions of the globe is not explicable by changes in the position of the poles, but is attributable to a higher temperature of the whole globe, whether due to external or internal causes, to the great amount of carbonic acid and water in the atmosphere, which would shut in and accumulate the sun's heat, according to the principles discovered by Tyndall and applied by Sterry Hunt in explanations of geological times, and possibly to a warmer position in stellar space, a more uniform distribution of surface temperature, and a different distribution of land and water.[1]

Before Count Saporta's essay had reached this country[2] another continuation of the subject of the origin of existing floras had been communicated to our own Geographical Society, by Mr. Thiselton Dyer in a lecture on "Plant Distribution as a Field for Geographical Research." Mr. Thiselton Dyer's order of procedure is the reverse of Count Saporta's, and his method entirely different. He first gives a very clear outline of the distribution of the principal existing floras of the continents and

[1] Professor Jos. Le Conte, in NATURE, vol. xviii. p. 668.

[2] Count Saporta's essay was presented to the International Congress of Geographical Science which met in Paris in 1875, and was not received in England till the autumn of 1878, though it bears date of 1877 on the title page.

islands of the globe, their composition, and their relations to one another, and to those of previous geological epochs. He then discusses the views of botanists respecting their origin and distinctive characters, and availing himself of such of their hypotheses as he thinks tenable, correlates these with those of palæontologists, and arrives at the conclusion "That the northern hemisphere has always played the most important part in the evolution and distribution of new vegetable types, or in other words, that a greater number of plants has migrated from north to south than in the reversed direction, and that all the great assemblages of plants which we call floras, seem to admit of being traced back at some time in their history to the northern hemisphere." This amount of accordance between the results of naturalists working wholly independently, from entirely different standpoints, and employing almost opposite methods, cannot but be considered as very satisfactory. I will conclude by observing that there is a certain analogy between two very salient points which are well brought out by these authors respectively. Count Saporta, looking to the past, makes it appear that the fact of the several floras which have flourished on the globe being succesively both more localised and more specialised, is in harmony with the conditions to which it is assumed our globe has been successively subjected. Mr. Dyer, looking to the present, makes it appear that the several floras now existing on the globe, are in point of affinity and specialisation, in harmony with the conditions to which they must have been subjected during recent geological time on continents and islands with the configuration of those of our globe.

(*To be continued.*)

HAECKEL ON THE LIBERTY OF SCIENCE AND OF TEACHING[1]

PROF. HAECKEL has recently published his reply to the address on "The Liberty of Science in the Modern State," delivered at last year's meeting of the German Association, by Prof. Virchow. If we enter into this subject at greater length than is our custom with pamphlets we do so mainly from a sense of common fairness to both parties, since we reproduced Prof. Virchow's address *in extenso* (NATURE, vol. xvii. pp. 72, 92, and 111). We shall, however, confine ourselves merely to stating Prof. Haeckel's views on the subject, and leave it to our readers to judge of the value of his remarks for themselves.

In the preface to the little book before us Prof. Haeckel states that the general views developed by Virchow are in such complete contrast to his own that no reconciliation of the two is possible. Yet he refrained for a considerable time from publishing his reply; and this for two reasons. On the one hand, he thought he might safely leave the judgment of the strife between them to the future; first, because the evolution theory, which Virchow attacks, has, *de facto*, become the basis of biological science of the present day; secondly, because Virchow's objections to the theory of descent have been so frequently and thoroughly refuted that it seemed superfluous to refute them again. On the other hand, he felt great reluctance in opposing a man whom a quarter of a century ago he honoured as the reformer of medical science, and whose pupil and zealous admirer he was for many years.

"The more I for years regretted Virchow's position as the enemy of our new evolution theory, and the more I was challenged to reply by his repeated attacks upon it, the less inclination I felt, nevertheless, to appear publicly as the antagonist of the highly-honoured and meritorious man. If now I find myself forced to reply, I do so with the conviction that longer silence would only augment the erroneous views which my resignation hitherto has already produced. . . . . I must point out distinctly that it is not Virchow but I who am the person attacked, and that in my case there is no question of *attacking* a formerly highly-honoured friend, but of *defending* myself by necessity against his repeated and violent attacks. Another reason which compels me to speak at last lies in the continued fertile use made of Virchow's speech by all clerical and reactionary organs for the last nine months. . . . Already Friedrich von Hellwald has pointed out the great danger which lies in the fact that it was a Virchow who, under the banner of political Liberalism, and wrapped in the mantle of pure science, combated the liberty of science and of its teaching."

The author then continues to point out that the danger was never so great in Germany as at the present moment, where the political and religious life of the German nation seems to approach a profound reaction. The two mad attempts upon the life of the honoured and aged Emperor have called forth a storm of just indignation of such violence that even many Liberal politicians not only press for severe measures against the utopian teachings of social democracy, but, far overshooting the mark, demand that free thought and free teaching should be confined within the narrowest bounds. What more welcome support can the reactionary party wish for than that a Virchow should publicly demand the suppression of the liberty of science? The danger appears still greater to Prof. Haeckel if Virchow's great influence as a "liberal Progressist" is taken into consideration, now that the Prussian Diet will shortly open its debates on the educational law. "What," Prof. Haeckel asks, "may we expect of this educational law, if in the discussions Virchow, as one of the few authorities who will be consulted, raises his voice in favour of the principles which in his Munich speech he proclaimed as the safest guarantees for the liberty of science in the modern state. Article 20 of the Prussian Constitution, and § 152 of that of the German Empire say: Science and its teachings are free. Virchow's first action, according to his present principles, must be a proposal to cancel this paragraph. In view of the menacing danger, I cannot hesitate any longer with my reply. Amicus Socrates, amicus Plato, magis amica veritas!" The rest of the preface is concerned with a refutation of the "denunciation" by which Virchow wants to make the theory of descent responsible for the horrors of the Paris Commune. Haeckel thinks that by an intentional coupling of Darwinism with Social democracy, Virchow intended to do considerable damage to the former, indeed he sees in it an attempt to remove all "Darwinists" from their Academical chairs. At the same time he points out that nine out of every ten zoologists and botanists now teaching at European universities are Darwinists. Virchow's attempt is therefore perfectly futile, and will certainly never have any effect at Jena. "What the Wartburg was for Martin Luther, what Weimar was for the greatest heroes of German literature, what Jena has been during three centuries for a large number of scientific men, that will Jena continue to be for the evolution-theory of the present day, as well as for all other scientific theories which develop freely, viz., a firm stronghold for free thought, free research, and free teaching."

We now come to the first chapter, which is headed "Evolution and Creation." The author remarks at the beginning that nothing has so greatly facilitated the progress of the evolution theory, as the fact that its principal problem, the question of the origin of species, was placed before the alternative: *Either* organisms have been *developed* naturally, in which case they must descend from the simplest and common ancestral forms—*or* this is not the case, and the different species of organisms have originated independently of one another, in which case they can only have been *created* in a supernatural way, *i.e.*, by a miracle. Natural development or supernatural creation of species—the choice must here be made, since a third way does not exist. Since Virchow and many other antagonists of the evolution theory constantly mix this up with the theory of descent, and this again with the theory of natural selection or Darwinism, Prof. Haeckel does not think it superfluous to give a concise definition of each of the three great theories at starting. He then states his definitions as follows:—"The relation of these theories according to the present state of science is therefore simply the following:—I. *Monism*, the universal theory of evolution, or the monistic pro-genesis theory, is the *only* scientific theory, which *rationally* explains the universe and satisfies the desire for causality in the human mind, since it brings all natural phenomena into a *mechanical* causal connection as parts of a great and uniform (*einheitlich*) process of development; II. *Transformism*, or the theory of descent, is an essential and indispensable part of the monistic evolution theory, because it is the *only* scientific theory which explains the origin of organic species in a rational manner, viz., by transformation, and reduces this transformation to *mechanical* causes; III. The theory of selection, or *Darwinism*, *is up to the present the most important one* amongst the different theories, which try to explain the transformation of species by *mechanical* causes; but it is by no means the *only one*. Even if we suppose that most species have originated through natural selection, yet we know, on the other hand, that many forms called species are merely hybrids from two different species and are propagated as such; at the same time we

[1] Freie Wissenschaft und freie Lehre. Eine Entgegnung auf Rudolf Virchow's Münchener Rede über "die Freiheit der Wissenschaft im modernen Staat." Von Ernst Haeckel.

can easily conceive that other causes may be acting in the formation of species, causes of which we have no idea at present. To decide what importance natural selection has in the origin of species is left to the judgment of the various naturalists, and in this question the authorities differ materially even to-day. Some ascribe a greater, others a smaller importance to it. But the different estimation of the value of Darwinism is quite independent of the absolute validity of the theory of descent, because the latter is up to the present the *only* theory which explains, in a *rational* way, the origin of species. If we abandon this theory then nothing remains but the *irrational* supposition of a miracle, of a supernatural '*creation*.' We will briefly designate this mystical belief in a creation as 'creatism.' In this decisive and inexorable alternative Virchow has now publicly stated his belief in creatism and his disbelief in transformism." The author then dwells at some length on this, and quotes from the *Zeitschrift für Ethnologie*, edited by Virchow and Prof. Bastian, in which the lattter applauds Virchow's Munich address, and ridicules Haeckel's "deliramenta and absurdities." There is no doubt, therefore, that Virchow has confessed himself a "dualist and creatist," and is as convinced of the truth of his principles as Haeckel is of the contrary as a "monist and transformist," yet the former still refrains from acknowledging his principles in all their consequences. "On the contrary he still clothes his opposition in the favourite phrase of the clerical papers, viz., that the theory of descent is an 'unproved hypothesis.' It is perfectly clear, however, that this theory will never be 'proved' if the proofs in existence to-day are not considered sufficient. How often has it been repeated that the scientific certainty of the theory of descent is not based upon this or that single experience, but upon the *totality* of biological phenomena, upon the *Causal-nexus* of evolution? What are we to think, therefore, of the new proofs for the theory of descent which Virchow demands?"

In Chapter II. Haeckel undertakes to give some "certain proofs of the theory of descent." He shows that all general phenomena of morphology and physiology, of chorology and oecology, of ontogeny and palæontology, can only be explained by the theory of descent, and reduced to *mechanical causes*. The guarantee of the truth of the theory lies particularly in the fact that the last simple causes are the same for all these complicated phenomena, and that other mechanical causes cannot be imagined. "Where are we, therefore, to find still further proofs for the truth of the theory of descent? Neither Virchow nor any other of the clerical antagonists or dualistic philosophers show us where we possibly might have to look for further proofs. Where in the world are we to find '*facts*' which speak louder than the *facts* of comparative morphology and physiology, the *facts* of rudimentary organs and of embryonal development, than the *facts* of palæontology and of the geographical distribution of organisms—in short, than all the known *facts* from the most various biological domains?" If proofs by actual experiments are demanded, these proofs have also been furnished by the domestication of animals and plants, and their variations under such domestication. All working naturalists are perfectly well aware to-day that the *morphological* value of the word "species" is not an absolute but a relative one; nor has it any physiological value. Haeckel points out here that the class of animals which furnish the best "certain proof" that the conception of species has only a relative value, are the sponges: "their liquid form wavers to and fro with extraordinary uncertainty and variability, and makes all distinction of species quite illusory." The species question, one of the principal points in the theory of descent, is not even mentioned by Virchow, and Haeckel considers this highly characteristic. He arrives at the conclusion that Virchow has never thoroughly digested the evolution theory, and has never attentively studied Darwin's principal work on the Origin of Species, nor any other work by this author. The remainder of this chapter is devoted to an account of Virchow's activity, first at Würzburg, and afterwards at Berlin. Virchow left Würzburg for Berlin in 1856; it was at the former university that he made the celebrated application of the cell-theory to pathology, which caused quite a revolution in the latter science. Haeckel considers that the exchange of the narrow sphere for the wider one was not beneficial in its consequences, and that the Virchow of the present day has completely changed from the Virchow of 1848-1856; he points out that only those who are aware of the enormous progress morphology has made during the last twenty years, and have followed this science in all its details, can estimate the full value of the theory of descent, and of Darwin's theory of selection. "Whoever wishes to convince himself of what an enormous revolution was caused by these theories, in comparative anatomy particularly, let him compare the classical 'Grundzüge der vergleichenden Anatomie' of Carl Gegenbaur (1870) and his 'Grundriss' (1878) with the older books on this science. Of all the colossal progress of morphology Virchow has no idea, since he was ever a stranger to this domain. His great reforms in pathology lie in the domain of physiology, and particularly in that of cellular physiology. But during the last twenty years these two principal branches of biological research have separated more and more. The great Johannes Müller was the last biologist who could comprise the entire domain of organic natural research, and who acquired immortal merit in both branches alike. After Müller's death (1858) the two halves fell apart. Physiology, as the special science of the functions of organisms, turned more and more towards the *exact* and experimental method. Morphology, on the other hand, as the science of the shapes and forms of animals and plants, could of course make but little use of this method; it was bound to have recourse to the evolution theory, and thus became essentially a *historical* natural science. It was just upon this historical and genetic method of morphology, in contrast with the exact and experimental method of physiology, that I laid a particular stress in my Munich address. If Virchow, in his counter address, had really refuted the latter in its various details, instead of fighting it with phrases and denunciations, this contrast of principles would at least have been worth minute examination. Yet I do not wish to reproach Virchow with this, since he is completely involved in the one-sided views of the school-physiology of the present day, and because morphology lies far too widely apart from his domain, to make it possible for him to judge its methods and ends for himself. If, nevertheless, upon every occasion he pronounces a depreciatory judgment upon it, then we must question his *competence* in doing this. It is true that in his Munich speech he prints with large type the phrase, 'That which adorns me is the *knowledge of my ignorance*.' But I regret that I must deny him this adornment most completely. Virchow does *not* know how ignorant he is of morphology. Otherwise he would not have pronounced those crushing sentences regarding it; he would not constantly designate the theory of descent as 'a hobby,' or 'dreaming, or as personal speculation, which now inflates itself on many domains of natural science.' Indeed Virchow honours me too much if he calls that my 'personal hobby' which for more than a decade has become the most valuable common property of morphological science. If Virchow were not so unacquainted with morphological literature he would know that it is already completely impregnated with the principle of descent; that in all morphological work which is carried on systematically and with a certain end in view, the theory of descent is now accepted as quite *indispensable*. But he is unaware of all this, and thus we understand why he continues to ask for 'certain proofs' for that theory when these proofs have in reality been furnished long ago."

Chapter III. treats of craniology as applied to the theory of the descent of man from the ape. Haeckel points out the importance of comparative and genetic craniology, but cannot help regretting that a great deal of time and labour has been wasted during the last ten years by "craniologists" with discussions as to the best method of measuring skulls, and also that numerous naturalists, Virchow among others, seem to have seen in "craniometry" the highest aim and object of craniological science, and to have forgotten what they really want to prove with their measurements. Turning to the question of the descent of man, the author remarks that the well-known phrase "man has descended from the ape," which is so often misunderstood and misapplied, can in the sense of the evolution theory but have this meaning: The human race as a whole has descended from the order of apes, *i.e.*, from one (or perhaps more) species of apes now long extinct; the latest ones in the long series of man's vertebrate ancestors were apes or ape-like animals. Of course none of the species of apes now existing can be regarded as the unchanged descendant of the old parental form. Virchow in his address expresses his doubt of the truth of the descent in question, while Haeckel looks upon it as one of the most certain phylogenetic hypotheses. He does not deny that the relative certainty of this as well as that of any other phylogenetic *hypothesis* cannot be compared to the *absolute* certainty of the *theory* of descent, and draws special attention to the great difference between the whole theory and any particular hypothesis relating to an individual order or class

of animals; such hypotheses are always dependent on our actual biological knowledge and may be changed at any time for better ones, while the theory as a whole needs no further proof; it is absolutely certain. But for the objective zoologist it is impossible, according to the principles of comparative systematics, to assign to man any other place in the animal kingdom than in the order of apes, or primates, as Linnæus calls them; this classification, which is inevitable, leads to the *common* descent of man and ape from *one* ancestral form; and this is the essential part of the question. The views as to the exact appearance of this ancestral form may be divided, but we must eventually arrive at the conclusion, if we consider all facts connected with the subject, that our long extinct ancestors can but have been real apes, *i.e.*, some placental mammal, which, if it existed to-day, we should certainly classify among apes. Finally, Haeckel points out how characteristic it is of Virchow's view on the matter that he again places palæontology into the foreground, and, before accepting the theory of descent, demands that an uninterrupted series of fossil transition forms between ape and man should first be found. As Darwin himself has minutely stated the reasons why the solution of this problem cannot be expected, and has shown the cause of the extraordinary incompleteness of the palæontological records, and of the natural impediments to a geological proof of the ancestral tree (in Chapter X. of the "Origin of Species"), Haeckel again arrives at the conclusion that Virchow has never attentively read Darwin's great work, and has never digested the teachings of palæontology.

Chapter IV. is entitled "Cell-Soul and Cellular Psychology." Haeckel states here that the views he expressed at Munich with regard to the soul of the cell, *i.e.*, "that we must indeed ascribe an independent soul-life to each organic cell," are but the natural consequence of Virchow's own teachings, viz., of the very fertile application which Virchow made of the cell theory to pathology. He then proceeds to give the definition of the word "soul" according to both philosophical theories, first according to the monistic or realistic theory, and then according to the dualistic or spiritualistic theory; he compares the simplicity of the former with the mystery and irrationality of the other. He adduces the various phrases in Virchow's address which leave no doubt on the subject that Virchow has completely abandoned the realistic theory in favour of the dualistic one, and shows the utter futility of Virchow's view that we cannot find psychic phenomena in the lower animals. "Volition and sensation, the most general and most indubitable qualities of all mental life, cannot be overlooked in the lower animals. Indeed, with most *Infusoria*, particularly with *Ciliata*, independent motion and conscious sensation (of pressure, heat, light, &c.) are so very evident, that one of their most patient observers, Ehrenberg, maintained up to his death that all *Infusoria* must have nerves and muscles, organs of sense and of mind (*Seelenorgane*) just like all higher animals.

"Now it is known that the enormous progress which science has recently made in the natural history of these low organisms has reached its climax in the maxim that they are *unicellular* (a maxim which Siebold pronounced thirty years ago, but which has been proved with certainty only recently); therefore in the *Infusoria a single cell* performs all the different functions of life, including the mental functions, which in the *Hydræ* and *Spongiæ* are divided amongst the cells of the two germinal lobes, and in all higher animals amongst those of the various tissues, organs, and apparatus of a complicated organism. . . . By the same right by which we ascribe an independent 'soul' to these unicellular *Infusoria*, we must ascribe one to all other cells, because their most important active substance, the protoplasm, shows everywhere the same psychic properties of sensitiveness (sensation) and movability (volition). The difference in the higher organisms is only that there the numerous single cells give up their individual independence, and like good state-citizens, subordinate themselves to the 'state-soul' which represents the unity of will and sensation in the 'cell-association.' We must distinguish between the central soul of the total polycellular organism or the 'personal soul' and the separate elementary souls of the single cells, or 'cell-souls.' This maxim is excellently illustrated by the interesting group of *Siphonophora*. There is no doubt that the whole *Siphonophora*-state has a very determined and uniform (*einheitlich*) will and sensation; yet each one of the single individuals which compose this state (or *Cormus*) has its separate personal will and sensation. Indeed each one of these is originally a separate *Medusa* and the 'individual' *Siphonophora*-state has resulted only by association and division of labour of this united society of *Medusæ*. Next to the unicellular *Infusoria* no phenomenon affords such ample and immediate proof for the truth of our cellular-psychology than the fact that the *human ovum*, like the ovum of all other animals, is a simple and single cell. According to our monistic conception of the cell-soul, we must suppose that the fertilised ovum already possesses *virtually* those psychic properties which in the particular mixture of parental peculiarities (*i.e.*, those of mother and father) characterise the individual soul of the new being. In the course of the development of the ovum the cell-soul of course developes itself simultaneously with its material substratum, and becomes apparent *actually* when the child is born. According to Virchow's dualistic conception of the 'Psyche,' we must suppose, on the contrary, that this immaterial being enters the soulless germ at some period of embryonal development (perhaps when the spinal tube separates from the germinal lobe?). Of course this way the pure *miracle* is complete, and the natural and uninterrupted *continuity of development* is superfluous."

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

ST. PETER'S COLLEGE, CAMBRIDGE, has made a statute assigning one of its Fellowships to the Jacksonian Professor. It is intended to limit this professorship by statute to some branch or branches of chemistry or physics, a specially constituted electoral body, including representatives of non-resident science, making the selection freely on each occasion of a vacancy.

THE site most favoured for the Sedgwick Memorial Museum, Cambridge, is Downing Street, in front of the present new museums. There will be a good opportunity of concealing from public view these extremely plain buildings and of erecting a satisfactory façade. The Sedgwick Committee have informed the University that 1,200*l*. is in their hands for this purpose, but this amount is insufficient, and the University, when better supplied with funds, must supply a good deal more. A Syndicate, including Drs. Paget and Humphry, Profs. Liveing, Newton, Hughes, and Colvin, has just been appointed to select a site, to obtain plans, to confer with the Sedgwick Committee, and report by midsummer next.

PROF. LEITH ADAMS, F.R.S., has been appointed to the Chair of Natural History in the Queen's University of Ireland, rendered vacant by the lamented death of Prof. Harkness.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 21.—"On a Method of Using the Balance with great Delicacy, and its Employment to Determine the Mean Density of the Earth," by J. H. Poynting, B.A., Fellow of Trinity College, Cambridge, and Demonstrator in the Physical Laboratory, Owens College. Communicated by Prof. Balfour Stewart, LL.D., F.R.S.

The two chief causes of error in the use of the balance are:—1. Disturbances through changes of temperature, such as convection currents, or unequal expansion of the two arms. 2. The possibility that after raising the beam on the supporting frame and lowering it again, the same parts of the knife edge may not come into contact with the planes. Errors from the first cause may be to a great extent avoided by protecting the balance with a gilded case, and reading the oscillations from a distance by means of a mirror on the beam. The residual effects may then be detected by taking three observations at equal intervals of time, the first and third having the same weights in the pan, and their mean being compared with the second (*i.e.*, for a short time the disturbance is assumed to be a linear function of the time). The second cause of error has been removed by not raising the beam between successive weighings. For this purpose a clamp is placed underneath one pan, which can be brought into action at any time to fix the pan in whatever position it may be. The weights can then be interchanged while the counterpoise (Borda's method being employed) maintains the beam in the same state of flexure, and the knife edges always remain in contact with the same parts of the planes.

The value of a given deflection was estimated by riders, and the weights were interchanged each by special arrangements.

The greatest deviation from the mean in the comparison of two 1 lb. weights in one group of twenty comparisons, when the weather was unfavourable, was 1·20 millionth of 1 lb., while in another group of twenty-seven comparisons (the weather being much finer and more favourable) the greatest error was 1·50 millionth.

To determine the mean density of the earth, a 1 lb. weight was hung from one arm of the balance at a distance of about six feet below it, and was accurately counterpoised in the other pan. A large sphere of lead (about 340 lbs.) was then alternately inserted under the hanging weight, and withdrawn. The difference which its attraction made in the weight of the hanging weight was about 1·45 millionth of 1 lb. This increase of the weight was measured as accurately as possible by means of riders on the beam of the balance. Comparing this with the attraction of the earth on the weight—that is, its weight—we can calculate the mass of the earth in terms of the mass of the lead sphere. The results hitherto obtained are only preliminary, though they seem near enough to former determinations to show that with improved arrangements which the author intends to make, a good value may ultimately be obtained. The mean of 11 determinations is 5·69, with a probable error of 0·15.

Physical Society, November 23.—Prof. W. G. Adams, president, in the chair.—Prof. Ayrton read a paper on the music of colour and of visible motion, which we give elsewhere.—Dr. Schuster then described his new method of adjusting the collimator of the spectroscope for parallel rays of different refrangibility. His plan is very simple, and is based on the fact that if the rays entering the prism are parallel, the focus seen in the telescope will remain constant when the prism is turned round, but if they are not parallel, the focus will shift. The process, therefore, consists in looking through the telescope while turning the prism. If the focus shifts, the collimator has to be adjusted until no shifting takes place. The adjustment must be made with a prism whose sides are perfectly plane, and a good one may be kept for the purpose.

Statistical Society, November 19.—A numerous list of candidates were balloted for and elected Fellows.—The Howard prize medal, with 20*l*., has been awarded to Surgeon John Martin, L.R.C.S. Edin., of the Army Medical Department, at present serving in India with the Royal Artillery. An *extra* prize medal has also been awarded to Capt. H. Hildyard, of the 71st Highland Light Infantry, his essay being scarcely inferior to that of Mr. Martin.—The President, Mr. G. J. Shaw-Lefevre, M.P., in his opening address, commented on the past work of the Society, especially the papers read by its members during the past session, and the various statistics collected through its operations. Their great object was to study the past so as to understand the present and be able to give a forecast of the future.—Prof. Jevons afterwards exhibited and explained to the meeting the arithmometer of M. Thomas, long in use among actuaries.

EDINBURGH

Royal Society, November 25.—The following office-bearers were elected:—President, Prof. Kelland; Vice-Presidents, David Stevenson, C.E., Bishop Cotterill, Sir Alexander Grant, Bart., David Milne Home, Sir C. Wyville Thomson, Prof. Douglas Maclagan; General Secretary, Prof. Balfour; Secretaries to Ordinary Meetings, Prof. Tait, Prof. Turner; Treasurer, David Smith; Curator of Library and Museum, Alexander Buchanan; Council, Prof. Fleeming Jenkin, Rev. R. Boog Watson, Dr. Hugh Cleghorn, Prof. T. P. Fraser, Prof. Rutherford, Dr. R. M. Ferguson, Rev. W. Lindsay Alexander, Dr. Thomas A. G. Balfour, J. T. Buchanan, Rev. Thomas Brown, Robert Gray, and Dr. William Robertson.

PARIS

Academy of Sciences, November 25.—M. Fizeau in the chair.—The following papers were read:—Critical examination of a posthumous writing of Claude Bernard on alcoholic fermentation, by M. Pasteur. He represents this writing as a sterile attempt to substitute for facts well established the deductions of an ephemeral system.—On the reduction in continuous fractions of $e^{F(x)}$, $F(x)$ designating an entire polynome, by M. Laguerre.—On isosceles figures, by M. Badoureau.—Reply to various communications by M. Levy.—Reclamation of priority, in regard to M. Werdermann's communication on an electric lamp, by M. Regnier.—On a new phenomenon of static electricity, by M. Duter. In certain cases electrification may change the volume of bodies. A large thermometric envelope containing water is made into a condenser by pushing a piece of platinum wire into its exterior, and fixing outside a sheet of tin. Whenever, like a Leyden jar, it receives a charge, the water is observed to descend, remain stationary while the charge persists, and resume its former level on discharge. It is inferred that the glass is dilated. With any kind of armatures the same result is had. Another experiment was to place the above-mentioned arrangement in another thermometric envelope containing water; on electrifying, the water in the measuring-tube of the outer envelope rose, while the other fell. M. Jamin recalled the fact that M. Govi, ten years ago, made an experiment similar to M. Duter's first, and attributed the effect to a contraction of the liquid; M. Duter's second experiment proves that the expansion of the glass is really the cause.—Reply to a note of M. Meunier on the artificial crystallisation of orthose, by MM. Fouqué and Levy. M. Meunier (they hold) had not sufficient data to determine the nature of the minerals produced; his experiments are a mere repetition of those of James Hall in 1798, who fused natural rocks, subjected them to long annealing, and found the metal grains obtained had sometimes a crystalline texture. The authors, far from having got results with orthose like those of M. Meunier, find a marked difference between this felspar and others as to its structure after reproduction by igneous fusion; it does not take the ordinary crystalline structure, and this reveals the necessity of intervention of volatile elements in genesis of acid rocks.—Note on the element called *Mosandrum*, by Prof. Lawrence Smith. He claims priority in having called attention to the absence of the oxide of cerium, and to new characters of certain earths in the mineral samarskite, and having indicated a new one he called *mosandrum*.—Double stars; certain groups of perspective, by M. Flammarion. He gives a list of couples that are merely optical groups, due to the meeting, in the same visual ray, of stars situated one beyond the other in space, and having different proper motions.—On the number of complete arrangements where consecutive elements satisfy given conditions, by M. André.—On various derivatives of spirit of turpentine, by M. De Montgolfier. He has studied the action of sodium chiefly in chlorhydrates of turpentine, solid and liquid.—On a cyanised derivative of camphor, by M. Haller.—Action of salts of chromium on salts of aniline in presence of chlorates, by M. Grawitz. He notes the advantage of using these salts in place of vanadic salts; they are less rare and dear, and have even greater energy. $\frac{1}{10}$ of a milligramme of bichromate of potash, per 125 grammes of aniline salt dissolved in water, still developes black.—On the physiological action of borax, by M. De Cyon. Borax added to meat to the extent of twelve grammes daily (a quantity ten times that required by the Jourdes process), may be taken in food without causing the least disorder in general nutrition. Substituted for marine salt, borax increases the power of assimilating meat, and may cause a large increase of weight in the animal, even when the alimentation is exclusively albuminoid. This all applies to pure borax only.

CONTENTS